TECHNICAL ASPECTS OF DETECTION AND INSPECTION CONTROLS OF A NUCLEAR WEAPONS TEST BAN

HEARINGS

BEFORE THE

SPECIAL SUBCOMMITTEE ON RADIATION

AND THE

SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY

CONGRESS OF THE UNITED STATES

EIGHTY-SIXTH CONGRESS

SECOND SESSION

ON

TECHNICAL ASPECTS OF DETECTION AND INSPECTION CONTROLS OF A NUCLEAR WEAPONS TEST BAN

PART 1 OF 2 PARTS

APRIL 19, 20, 21, AND 22, 1960

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TECHNICAL ASPECTS OF DETECTION AND INSPECTION CONTROLS OF A NUCLEAR WEAPONS TEST BAN

TUESDAY, APRIL 19, 1960

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION AND
UBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittees met, pursuant to call, at 10 a.m. in room P–63, the Capitol, Representative Chet Holifield (chairman of the Special Subcommittee on Radiation) presiding.

Present: Representatives Holifield, Price, Hosmer, Bates, and Westland; Senators Pastore, Gore, Hickenlooper, and Aiken.

Also present: James T. Ramey, executive director; John T. Conway, assistant director; David R. Toll, staff counsel; George F. Murphy, Jr., professional staff member; Edward J. Bauser, technical adviser; Richard T. Lunger, staff consultant; Doyle L. Northrup, technical consultant; Joint Committee on Atomic Energy.

Representative HOLIFIELD. The committee will be in order.

This is a joint hearing of two subcommittees of the Joint Committee on Atomic Energy—the Subcommittee on Research and Development and the Special Subcommittee on Radiation. In accordance with our regular practice, all members of the full committee have been invited to attend and to participate whether or not they are members of the subcommittees holding the hearing.

We begin this morning the first day of public hearings on the technical problems of a nuclear weapons tests ban and the necessary detection techniques and inspection controls. We have arranged for a number of leading U.S. scientists and technical experts to discuss in public the effectiveness of the planned control system and the problems we may have to face.

It is well known that the Congress, the public, and even the executive branch have been subjected to conflicting statements as to various technical problems that may or may not exist. To whatever extent it is possible, it is our intention—through these hearings—to make public in an understandable manner all available information pertinent to this subject.

We hope that our witnesses will be able to separate the known from the unknown, and within the unknown to separate the possible from the impossible, and the probable from the improbable. We hope they will testify as much as possible in simple layman language easily understood by all.

Negotiations between the United States, the United Kingdom, and the U.S.S.R. have been in progress since October 31, 1958—a period of over 17 months—in an effort to reach agreement on a treaty for the cessation of nuclear weapons testing under an effective control system.

Previously, from July 1 through August 22, 1958, technical experts from the West had met with experts from the East to recommend an effective control system.

It was the unanimous report of the Conference of Experts, issued August 20, 1958, which became the basis for the diplomatic negotiations which began October 31 of that same year. Subsequently, however, data developed from the Hardtack series of tests proved some of the technical conclusions of the Conference of Experts relating to underground testing were in error.

It is our intention during the hearings beginning today to review what has been called the Geneva System recommended by the Conference of Experts, as well as the additional data subsequently developed. We will ask our scientific and technical witnesses to discuss for us in layman's language how a control system would operate and how effective it would be under our present state of knowledge.

We will look into the various methods of detecting and identifying possible violations under various methods of testing whether it be in the atmosphere, underground, underwater, or what is termed high altitude. We will look into and try to ascertain all methods of improving detection and inspection techniques and possible methods wherein a potential violator would attempt to cheat.

It is important that all presently available facts on this subject be brought to the attention of the Congress and the public. If and when a treaty is proposed, we should have an informed Congress and public capable of understanding what a treaty involves. Also, if we are to succeed in having an effective control system, it is important that we know and understand what the research program may include to improve the detection system. We should know this now so we can begin this research as soon as possible.

I should like to emphasize that these hearings will be concerned primarily with the technical aspects of a test ban. We desire to avoid as much as possible any discussion of policy questions as to whether a test ban fits into a desirable system of disarmament. There may be policy questions on a test detection system, as such, which we should and must discuss. Such discussions, however, should be confined to the technical possibilities of the problem, if possible.

We also hope to avoid emotional or philosophic discussion of the effects of nuclear testing in terms of radioactive fallout. The technical aspects of the effects of fallout have been considered rather extensively in previous hearings.

In order that the record may be complete and without objection, I will submit for the record a statement made April 11, 1960, by Senator Clinton P. Anderson, chairman of the Joint Committee, in which he announced these hearings.

(The statement referred to follows:)

PUBLIC HEARINGS ON NUCLEAR WEAPONS TEST CESSATION

Public hearings will be held on the problems of nuclear weapons test cessation and the necessary inspection controls by two subcommittees of the Joint Committee on Atomic Energy, it was announced today by Senator Clinton P. Anderson.

The hearings to be held jointly by the Subcommittee on Research and Development and the Special Subcommittee on Radiation will begin April 19, 1960, in the old Supreme Court chamber (room P–63), U.S. Capitol Building, and are expected to run for several days.

In making the announcement, Senator Anderson, who is chairman of the Joint Committee, stated:

"Negotiations between the United States and the United Kingdom on one hand and the U.S.S.R. on the other have been going on in Geneva since October 31, 1958—over 17 months—in an effort to reach agreement on the cessation of nuclear weapons tests with adequate controls. Numerous conflicting stories have come out as to various technical problems that may or may not exist in setting up an adequate control system. It has been extremely difficult for the public to obtain accurate and complete information as to what type of control system is contemplated and what the technical problems may be. These hearings are being called in order that all presently available facts may be brought to the attention of the public. If a treaty is proposed, it is necessary that we have an informed public capable of understanding what the treaty involves."

Senator Anderson went on to advise that leading American scientists are being called to testify. Included will be Dr. Wolfgang H. Panofsky and Dr. Hans Bethé of the President's Science Advisory Committee, who have participated in technical conferences in Geneva in an effort to establish a proper control system. Dr. Edward Teller, Dr. Alvin Graves, and Dr. Harold Brown, associated with the weapons program, will also testify. A panel of scientists including those who have expressed special interest in this subject such as Dr. Jay Orear and Dr. Harrison Brown [1] as well as a number of seismologists will discuss the capabilities of monitoring for violations during a nuclear test moratorium.

Senator Anderson stated:

"These hearings will be conducted in the same careful, objective manner as the hearings held by Congressman Chet Holifield on the nature of radioactive fallout and its effects on man. Universities and scientific groups throughout the world are using those hearings as text reference books. Experienced, knowledgeable scientists and other experts will be invited to testify and discuss their concepts and opinions on problems of nuclear test cessation and detection techniques."

A detailed outline of the hearings and a complete list of the witnesses is expected to be ready by Wednesday.

Representative HOLIFIELD. I will also submit a copy of a joint statement by Congressman Melvin Price, chairman of the Subcommittee on Research and Development, and myself as chairman of the Special Subcommittee on Radiation, issued on April 17.

(The statement referred to follows:)

LIST OF SCIENTIFIC WITNESSES AND OUTLINE OF PUBLIC HEARINGS APRIL 19–21 FOR NUCLEAR WEAPONS TEST CESSATION

Eighteen leading nuclear scientists and seismologists who will testify at 3 days of Joint Committee hearings on nuclear test detection beginning Tuesday, April 19, 1960, were announced today by the chairmen of the Special Subcommittee on Radiation and Subcommittee on Research and Development of the Joint Committee on Atomic Energy.

In releasing the proposed hearings scheduled and list of witnesses, the chairmen of the two subcommittees, Congressman Chet Holifield and Congressman Melvin Price, issued the following joint statement:

"These hearings are being called in order that all presently available facts may be brought to the attention of the public. It is of utmost importance that we have an informed public capable of understanding what a treaty or international agreement involves should one be proposed. A number of conflicting statements have been made as to various technical problems that may or may not exist in the operation of an adequate control and detection system. As we have done in the past in our hearings we intend to bring together a representative group of the leading scientists and technical experts in these areas so that through their testimony and panel discussions we may determine the facts."

Among those scheduled to testify are the following: Dr. Lloyd V. Berkner,[2] Dr. Wolfgang H. Panofsky, Dr. Hans A. Bethe and Dr. Edward Teller.

Congressmen Holifield and Price concluded by stating:

"We wish to emphasize that these hearings will be concerned with the technical aspects of a test ban, and particularly to consider the type of research and development programs necessary for a workable system of detection and inspection."

A detailed outline of the hearings and a complete list of the witnesses is attached.

Representative HOLIFIELD. I will also at this point in the record, and without objection, introduce an outline of the hearings with the

[1] Dr. Harrison Brown notified the committee he was unable to attend.
[2] Dr. Berkner notified the committee he was unable to attend.

names of the witnesses and the specific topics which they will discuss. Also as an appendix to these hearings will be a "Glossary of Terms," which I believe will be very helpful to the layman reading this publication. (See attachment 1, p. 419.)

(The outline referred to follows:)

JOINT COMMITTEE ON ATOMIC ENERGY

SPECIAL SUBCOMMITTEE ON RADIATION AND SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT

PROPOSED OUTLINE FOR PUBLIC HEARINGS ON NUCLEAR TEST DETECTION, APRIL 19–21, 1960*

TUESDAY, APRIL 19, 1960

Morning session: 10 a.m. Afternoon session: 2 p.m. Location: Room P–63, old Supreme Court chamber, the Capitol Building.

1. An explanation of the Geneva system and what it is designed to accomplish. This discussion is to include the history of the Conference of Experts.
   Witness: Dr. Harold Brown, Lawrence Radiation Laboratory, Livermore, Calif.
2. A presentation of the results of Technical Working Group I (High Altitude).
   Witness: Dr. Wolfgang H. Panofsky, High Energy Physics Laboratory, Stanford University, Palo Alto, Calif.
3. A presentation of the results of Technical Working Group II (New Data).
   Witnesses: Dr. Wolfgang H. Panofsky, Dr. Harold Brown.
4. Discussion of a Geneva type international control system now possible under the present state of knowledge.

A. Capabilities and limitations for detection of underground tests.

(1) Numbers of events detected and identified as a function of signal strength.
   Witnesses: Dr. Carl F. Romney, Air Force Technical Applications Center, Washington, D.C. (AFTAC) and Dr. Richard Latter, the Rand Corp., Santa Monica, Calif.

(2) Means of improvement.
   Witness: Dr. Carl F. Romney.

(3) Means of circumventing detection (decoupling).

(*a*) The theory of decoupling and the results of the Cowboy series.
   Witness: Dr. Albert R. Latter, Rand Corp., Santa Monica, Calif.

(*b*) The feasibility of construction of large cavities—cost—time scale.
   Witness: Mr. L. P. Meade, Phillips Petroleum Co.

(*c*) Additional techniques of decoupling.
   Witness: Dr. Edward Teller, Lawrence Radiation Laboratory, Livermore, Calif. and Dr. Hans A. Bethé, Cornell University, Ithaca, N.Y.

WEDNESDAY, APRIL 20, 1960

Morning session: 10 a.m. Afternoon session: 2 p.m. Location: Room P–63, old Supreme Court chamber, the Capitol Building.

4. Discussion of a Geneva type international control system now possible under the present state of knowledge (continued).

B. Capabilities for detecting tests in the atmosphere at high altitude and in space.

(1) General discussion of capabilities.
   Witnesses: Dr. Wolfgang H. Panofsky, Dr. Richard Latter, Rand Corp., Santa Monica, Calif.

(2) Satellite vehicle requirements for reporting the system.
   Witness: Dr. Allen F. Donovan, Space Technology Laboratory, Los Angeles, Calif.

(3) Time scale data for achieving the capability.
   Witness: Dr. Richard Taschek, Los Alamos Scientific Laboratory, Los Alamos, N. Mex.

C. Requirements (dollars and manpower) for the underground and space systems.
   Witness: Mr. Carleton M. Beyer, Advanced Research Projects Agency, Department of Defense.

5. Procedures, capabilities, techniques and limitations of onsite inspection.

A. Procedures and techniques of onsite inspection.

*A full list of witnesses in order of appearance will be found in attachment 2, p. 424.

(1) Team phasing.
(2) Individual techniques.
(3) Future programs and requirements.
Witness: Dr. Richard M. Foose, Department of Earth Sciences, Stanford Research Institute, Menlo Park, Calif.
B. Capabilities.
1. Methods of testing.
2. Nature of zones underground.
Witness: Dr. Charles E. Violet, Lawrence Radiation Laboratory, Livermore, Calif.
C. Methods of detection of cavities.
Witness: Dr. Roland Beers, Consultant in Seismic Techniques, Troy, N.Y.

THURSDAY, APRIL 21, 1960

Morning session: 10 a.m. Afternoon session: 2 p.m. Location: To be announced.
6. Critique of capabilities of monitoring nuclear tests during a moratorium.
Panel members:
Dr. Roland Beers, seismic consultant, Troy, N.Y.
Dr. Hans Bethé, Physics Department, Cornell University, Ithaca, N.Y.
Dr. Harold Brown, Lawrence Radiation Laboratory, Livermore, Calif.
Dr. Dean S. Carder, Chief Seismologist, U.S. Coast and Geodetic Survey, Washington, D.C.
Dr. Alvin C. Graves, Los Alamos Scientific Laboratory, Los Alamos, N. Mex.
Dr. Richard Latter, Rand Corp., Santa Monica, Calif.
Dr. Jack E. Oliver, Lamont Geological Observatory, Columbia University, New York, N.Y.
Dr. Jay Orear, Physics Department, Cornell University, Ithaca, N.Y.
Dr. Richard E. Roberts, Carnegie Institution, Washington, D.C.
Dr. Carl F. Romney, Headquarters USAF, Air Force Technical Applications Center, Washington, D.C.
Dr. Edward Teller, Lawrence Radiation Laboratory, Livermore, Calif.
Dr. Harold Urey, University of California, Scripps Institute of Oceanography, La Jolla, Calif.
7. Program requirements for any agreed on period of "cooperative research" to include:
A. General research program requirements.
Witnesses: Dr. Richard Latter, Mr. Carleton M. Beyer.
B. Requirements for chemical and/or nuclear detonations to include schedules and costs.
(1) Basic criteria.
Witness: Dr. Richard Latter.
(2) Experimentation schedules and costs.
Witness: Dr. Gerald W. Johnson.
(3) Seismic instrumentation requirements.
Witness: Dr. Carl F. Romney.
C. Requirements for detection equipment and techniques to include schedules and costs.
(1) Seismic.
Witness: Dr. Carl F. Romney.
(2) High altitude detection techniques.
Witnesses: Dr. Richard Taschek, Dr. Richard Latter.

Representative HOLIFIELD. Every effort has been made to review the overall pertinent technical areas and to select our witnesses on the basis of their special knowledge of the individual topics they will be discussing. As the hearing progresses, we may find additional topics that should be covered and additional witnesses with special competency in those areas. The Chair accordingly reserves the right to augment the outline and to call additional witnesses at the direction of the committee.

Following each witness' oral testimony and the question and answer period, we will place his formal statement in the record. Also we will insert a biographical sketch of each witness.

Our first witness this morning will be Dr. Harold Brown of the Lawrence Radiation Laboratory, Livermore, Calif. Dr. Brown, who was one of the American representatives at the Conference of Experts in Geneva July and August 1958, will discuss the control system recommended by the Conference and discuss what it is designed to accomplish.

Dr. Brown, we are pleased to have you with us this morning and you may proceed.

STATEMENT OF DR. HAROLD BROWN,[1] LAWRENCE RADIATION LABORATORY, LIVERMORE, CALIF.

Dr. BROWN. Mr. Chairman and members of the committee, it is a great pleasure to appear before you. The purpose of this talk is to explain the matters which were considered by the Conference of Experts which met in Geneva the summer of 1958 and to outline the conclusions which that conference reached (app. 1, p. 479). I will also say something about the uncertainties which were recognized to exist at the time and the matters which were then omitted from consideration for a variety of reasons.

The Conference of Experts considered a number of techniques for the detection and identification of nuclear explosions, attempting to evaluate them on the basis of what was then known. These methods include the air acoustic and hydroacoustic signals, the collection of radioactive debris from the atmosphere, the electromagnetic signal, and the seismic waves from an explosion.

They also include the detection of nuclear radiation by earth satellites and the measurement of ionospheric disturbances for the detection and identification of nuclear explosions high in the atmosphere or in deep space.

During the Conference of Experts it became apparent, as was no surprise to anyone that the Soviets were in favor of as small a number of stations and as minimal an accessibility by the control organization to the Soviet Union as they could get the Western delegation to agree to.

Their action in this direction took the form of exaggeration of the capabilities of each of the methods for detection. By producing experimental evidence or more accurate theories than the Soviets, the Western delegation was able to get its evaluation of the individual capabilities agreed to by the Soviets, although the language was not always as clear as could have been hoped had the Soviets been operating on a purely technical basis.

The Soviet delegation did finally agree to a number of points which are important as matters of principle. They did agree to the dispatch of special flights for the detection of possible radioactive clouds where there were acoustic or electromagnetic indications of possible nuclear

[1] Born in New York in 1927. He received the A.B. degree in 1945, the A.M. in 1946, and the Ph. D. in 1949 from Columbia University. He was a lecturer and staff member at Columbia from 1947 to 1950, and lecturer at Stephens Institute of Technology in 1949–50. In 1950 he joined the staff of the University of California Radiation Laboratory, Berkeley, and was a lecturer in physics in 1951–52. Went to UCRL Livermore in 1952 as an original member of the laboratory's staff, became a division leader in physics in 1955, was appointed an associate director in 1958, and in 1959 was appointed deputy director of the Lawrence Radiation Laboratory at Livermore.

explosions. It was also agreed that completely contained underground nuclear explosions, although they could be detected down to some yield by this system, could not be identified by seismic readings.

Consequently, the report indicated that for seismic events not identified as earthquakes by the control posts, inspection at the site of the event would be necessary in order to identify it either as an explosion or as an earthquake.

No agreement was reached at that time on recommendations for the apparatus to be used for the detection of nuclear explosions in the outer atmosphere or in space, although certain statements were made about the capabilities of some of these methods.

The report of the conference concluded that for nuclear weapons explosions of one kiloton or more in the atmosphere up to about 50 kilometers and for nuclear explosions in the oceans there would be a fairly high probability of identification on the basis of the remote signals, together in some cases with onsite inspection.

This capability rests principally on the air-acoustic and radioactive debris techniques for the explosions in the atmosphere, supplemented in the case of explosions on the surface by an onsite search for radioactivity. The electromagnetic signal, although useful as confirmatory evidence, can be greatly decreased by surrounding the explosion with tons of shielding material, and, therefore, its absence does not constitute evidence that the event is not a nuclear explosion.

There are areas over the oceans where the net spacing is rather sparse and where atmospheric explosions of 1 or even several kilotons might often fail to be detected. However, as a general rule the statement on capability for these atmospheric explosions corresponded to a rather high probability of detection and identification.

Thus, for the acoustic method the spacing of 1,700 kilometers gave probabilities of 70 to 90 percent detection of the acoustic signal of a kiloton explosion at enough stations to locate the event. In addition, the radioactivity present in the atmosphere or in the ocean would, with substantial probability, provide confirmation.

This technique would enable the natural events occurring with fair frequency—perhaps 100 annually worldwide of 1 kiloton acoustic equivalent—which simulate the acoustic signals by nuclear explosions to be eliminated by a cursory onsite inspection. These events are principally meteors or volcano eruptions. It is thus apparent that for explosions in the atmosphere there would be a substantial probability—of the order of 50 percent—of detection and identification in the range of 1 or 2 kilotons even if elaborate attempts were made at concealment.

This may not be true everywhere in the world. It may not be true in the areas of the ocean where there is no land to put stations on, but it is by and large correct.

I mention all of this as background to indicate the kind of identification probability that was considered adequate for control in the case of atmospheric explosions.

In the case of explosions in space, an estimate was made of the capabilities of the earth satellite system for the detection of nuclear explosions. It was estimated that unshielded nuclear explosions of 1 kiloton could be detected at a distance up to hundreds of thousands of kilometers by their gamma radiations. Shielding was known to be able to reduce this capability, but no precise estimate was made.

The exact range of detection was known to depend on the backgrounds of radiation which would be encountered by the satellites in the vicinity of the earth, about which not very much was known in 1958 except the existence of the Van Allen Belt, together with some very preliminary ideas of its nature.

As to underground nuclear explosions, everything was normalized or calibrated to the coupling, that is, the degree of energy transfer into seismic waves, observed in the Rainier explosion in September 1957 which was the only underground nuclear explosion carried out before the Conference of Experts so far as we know.

Representative HOLIFIELD. Could you elaborate on that particular explosion, tell what it was and what type of earth it was exploded in?

Dr. BROWN. Yes, sir. It was a 1.7-kiloton explosion, exploded approximately 800 or 900 feet below the surface at the Nevada test site. The material in which it was exploded is known as tuff. It is a potassium aluminum silicate which is porous, but the pores are filled with water.

As I will mention later, explosions in different types of material would be expected to behave differently.

Representative HOLIFIELD. Then in the 1958 conference you had before it the results of that particular explosion?

Dr. BROWN. Yes, sir.

Representative HOLIFIELD. An underground explosion?

Dr. BROWN. Yes, sir.

Representative HOLIFIELD. Did it have before it any other underground explosion?

Dr. BROWN. There was sparse evidence on large explosions of a chemical nature, none of which, however, was completely contained; that is, they were always close enough to the surface so that they would vent into the atmosphere. This could produce, and we believe it did produce, quite different seismic signals.

In addition, many of the chemical explosions that had taken place before then, had been extended charges; that is, they extended over hundreds of yards and in some cases over 1,000 yards. This again makes a quite different coupling into the various kinds of seismic waves.

Although the Soviets did present some evidence on these chemical explosions, I think we were able to convince them that the most applicable data which existed at that time were from the Rainier explosion, and by and large I would say that the experts' report was based principally on those data, which included such things as the size of the seismic signal as a function of the distance from the explosion, the frequency of the seismic waves from the explosion, and so on.

Representative HOLIFIELD. We understand that the Soviets have had quite a number of chemical-type underground explosions over the past few years. Did they make available to the conference their data on their tests?

Dr. BROWN. By no means complete data. I think it is true that they showed some seismic traces from some of these explosions. But there has been nothing like complete access to that data in contrast with what has been the case with the seismic data from our underground explosions.

Representative HOLIFIELD. The United States had made available its underground explosion data, both chemical and nuclear?

Dr. BROWN. Yes, sir.

A footnote in the report of the Conference of Experts pointed out that smaller couplings than this might be possible. That is to say, the same yield explosion might be made to give smaller seismic signals. If so, then all the statements made about 5 kilotons in that report, for example, would correspond to yields of underground explosions equal to 5 kilotons times whatever the factor of reduction in coupling was.

So if the factor of reduction in the coupling was five, that would correspond to 25 kilotons. If the factor of reduction in coupling was a hundred, it would correspond to 500 kilotons.

At the time of the Conference of Experts, it was believed that other media might have coupling less than Rainier or more than Rainier by a factor of a few, depending on how the properties of the medium compared with those of Nevada tuff in which the Rainier explosion took place.

Representative HOLIFIELD. At this point would you—maybe you do it later in your statement—would you explain for the record the difference in recording of a test in granite, let us say, and in tuff, and in volcanic ash and in other strata of earth formation?

Dr. BROWN. Sir, it is a little difficult to say because we have not yet had nuclear explosions underground in anything but this volcanic tuff. People have looked at this theoretically. I must say that their conclusions are very tentative because this is an area in which the behavior of materials is not very well known.

This results from the inelastic behavior of materials when the pressures are high enough to make them crack and crush and even in some cases melt. However, some preliminary theoretical work has been done (it would be very valuable to have experiments to try to verify this). It indicates that if one goes to other kinds of rock, for example granite, one might—and I say only might, so as to emphasize the preliminary nature of these calculations—expect that at a distance of several hundred kilometers, for example, the amplitude of the seismic waves that were seen would be a fraction, perhaps a third or so, of what they were in tuff.

Representative HOLIFIELD. Do you base this on chemical-type explosions in different types of soil, or is this a theoretical calculation?

Dr. BROWN. We base it principally on theoretical calculations. However, there are some experiments with chemical explosions. I think one must be very cautious in extrapolating these on this particular point.

Representative HOLIFIELD. We have a section in the hearings that will be devoted to this subject, we will go into this in more detail at that time.

Dr. BROWN. Yes, sir. I think I should say that insofar as that evidence says anything, it does say that there is a factor of two or three difference between tuff and salt, which is another example of hard rock when closely coupled.

The thought that decouplings of a large factor, of the order of hundreds, might exist had occurred to many of us, but no clear understanding existed at that time about what the modification of the medium would be necessary in order to make this decoupling take place. In the absence of such understanding, the possibility of large decoupling was pretty generally ignored.

The Conference concluded that the sytem would be able to locate, in seismic regions where the separation of stations was 1,000 kilometers, most earthquakes or underground explosions whose seismic signals were greater than or equal to that from a 1-kiloton nuclear explosion with Rainier coupling.

In seismic regions, where the spacing is rather larger—1,700 kilometers—one can calculate that the yield required for a good probability of location, on the basis of information available to the Conference, is about 5 kilotons equivalent yield. For location purposes, one needs to be able to obtain a clear time of arrival from at least three or four seismic stations.

These estimates are based on that number of stations detecting the arrival of the event. Failing this criterion, one can perhaps obtain wiggles on one or more seismic graphs which are due to the seismic event—be it earthquake or explosion—but it cannot be said to be detected in any true sense if its position is unknown.

Representative HOLIFIELD. I might say we have a blackboard available in case you want to use it at any time, Dr. Brown.

Dr. BROWN. Thank you very much, sir.

Senator GORE. May I ask a question, Mr. Chairman?

Representative HOLIFIELD. Senator Gore?

Senator GORE. Doctor, you speak of location and then you say it cannot be said to be detected in any true sense if its position is unknown. If you, by three seismographic installations, are able in the terms in which you speak here to locate an underground explosion, how exact do you mean that the location could be determined?

Dr. BROWN. I can go into a little detail on that later, but let me say something about it now. In principle, one can locate to the order of a radius of 5 to 8 kilometers, perhaps. The area of uncertainty will not be a circle. It is a few hundred square kilometers.

Senator GORE. An area 5 miles in diameter?

Dr. BROWN. About 5 miles in radius, sir. But that is only in principle.

Senator GORE. It would be 10 miles in diameter?

Dr. BROWN. That is correct. In fact, as I will mention later on, the area may be considerably larger than that if the anomalies in the travel time of seismic waves are not well known, which is the case in most places. So let me say this: The area would be somewhere between 100 square miles and 400 square miles.

Senator GORE. 100 and 400 square miles?

Dr. BROWN. Yes. That is not quite what the Experts' Conference report says, but it says almost that. It says that the area is 100 or 200 square kilometers (and 200 square kilometers is about 100 square miles), providing that one can surround the event with a large number of stations.

Senator GORE. So what you really mean is that by instrumentation you can locate an area in which a suspicious event has occurred?

Dr. BROWN. Yes, sir.

Senator GORE. An area varying in size from 100 to 400 square miles?

Dr. BROWN. That is correct.

Senator GORE. Then to bring about an exact location, this particular area must be searched by an on-the-spot inspection team?

Dr. BROWN. That is correct, sir.

Senator GORE. How would they search?

Dr. BROWN. There is a whole session on this this afternoon or tomorrow, but I can say a little bit about it if you would like to hear something about it now.

Senator GORE. I will wait and let it come in its proper order. I was curious about your use of the term "location" and exactly what you meant.

Dr. BROWN. It is a very important point that you bring out.

Representative HOLIFIELD. I think you can answer the Senator's question in a brief sentence.

Dr. BROWN. Surely. The idea would be to use geophysical methods of prospecting to try to locate more accurately. Also, to look for unusual human activity, which is a detective rather than a scientific activity.

In the geophysical method you would do such things as look for broken areas and to see if you can see disturbed areas below the surface. The other general method is to look for signs of unusual human activity, which everyone defines in a different way.

Senator GORE. By "unusual human activity" you mean a road built to a tunnel under the mountain?

Dr. BROWN. For example, if that is an unusual human activity. There are lots of mines.

Senator GORE. I will not press you on "unusual human activities." There are many. I do not know of many norms.

Dr. BROWN. Now one cannot tell from seismic signals that an event is an underground explosion. It is, therefore, important to note that there are a very large number of seismic events annually which gives signals equal in magnitude to that produced by a nuclear explosion of one kiloton or more with Rainier coupling.

One then asks how many of the seismic signals produced by earthquakes can be identified as such by the system of seismic detection posts, so as to find out whether one can keep the system from being swamped by the number of events which could be nuclear explosions.

The Conference of Experts tried to find a number of such possible distinguishing characteristics. About half of the earthquakes may be eliminated on the basis of their taking place underneath the ocean floor where the depth of the floor is presumably too deep for nuclear explosions to be carried out in any foreseeable way.

There are also a number of earthquakes on the continents whose depth of focus can be established as being too deep for humans to have reached. However, these amount at present to only about 10 percent. The Conference of Experts adopted as a principal technique which the system would use to identify earthquakes, as such, the so-called criterion of first motion. It is generally considered—and almost an article of faith among seismologists—that a large enough underground explosion should be characterized by a positive first motion at all seismic stations which detect it. In other words, an explosion should produce an initial outward motion in all directions which should be seen provided that the seismic signal is large enough.

Senator GORE. Those are not unusual human events?

Dr. BROWN. No, sir.

Representative HOLIFIELD. Would you show on the blackboard what you mean by this outward explosion?

Dr. BROWN. Yes, sir.

Here is the surface of the earth [indicating on the blackboard] and here is, let us say, an explosion. The earth is supposed to move outward in all directions from the explosion.

What actually happens is that there is a discontinuity some tens of kilometers below the surface and a wave goes down like this, along the discontinuity and back up again. If there is a seismic station up here, this should produce an upward motion. That is what people mean when they say a positive first motion.

Representative HOSMER. Will you draw a line up above to show the curve of the seismic disorder?

Dr. BROWN. Yes, sir. It would look roughly like this and that would be the positive first motion. That would presumably be the same in all directions and at all distances. Theoretically, that is. I will say a little more in a moment about what actually happens.

Representative HOLIFIELD. Put stations at the four corners and we will assume they are the required distance of 1,000 kilometers apart.

Dr. Brown. Yes, sir.

Representative HOLIFIELD. Will you explain at this time, leaving your illustration as it is, how you would detect the location of the event?

Dr. BROWN. If one looks down, here is the source and here are some stations. One sees the time at which this wave arrives and one knows, not terribly well—that is what causes the position uncertainty—the velocity with which the seismic waves move.

One can, therefore, draw a circle—it is not a circle; it is a hyperbola, I suppose—for each pair of these stations and say that that pair of stations tells you that the source lies somewhere on that line. You will get several such curves and where they intersect—and they do not intersect at a point because you do not know this velocity well enough—that is the approximate location of the event.

This is very much like sound ranging in artillery fire, for example. If you can hear a thing from a number of points and you know the velocity of the sound wave you can tell roughly where the sound wave originated. The same thing is roughly true for these.

For an earthquake, what is supposed to happen is that there is a slip like that. The ground above moves this way and that below moves in the other direction. What that means is that in some directions you will have again a positive upward first motion. However, in other directions, out here for example, what you would see is this, where the ground is moving down and you will have a negative first motion and that curve will look like this.

Let me repeat, this is the theory. It is, in fact, much more complicated.

Representative HOLIFIELD. Between the two there, at the initial starting of the motion in the case of the earthquake, it is a downward motion on the seismograph?

Dr. BROWN. On some seismographs.

Representative HOLIFIELD. And on the nuclear explosion it is an upward motion?

Dr. BROWN. On any explosion it is supposed to be upward.

Representative HOLIFIELD. Any nuclear or chemical, it is an upward motion?

Dr. BROWN. In principle; that is correct.

Representative HOLIFIELD. So unless you catch the first initial impulse, from that time on you are at a loss?

Dr. BROWN. It is, in fact, worse than that, because if you miss the first impulse, which is in general much smaller than the second one, then what you see is the second one. If you are looking at an explosion and you miss the first one and see the second one, you will say it is a negative first motion, which is characteristic of earthquakes. That is a way to identify an explosion as an earthquake, just by missing the first motion.

Representative HOLIFIELD. This has to be observed in relation to background noise which occurs in all types. There is a certain amount of up and down motion on your seismograph line at all times, is there not, which represents background noise?

Dr. BROWN. Yes, sir.

Representative HOLIFIELD. Unless this initial impulse is stronger than the background noise in that area, it would be confused or even go unnoticed?

Dr. BROWN. As you can see from the curve I have drawn in to represent the background noise, here is a case in which the first motion will have been missed, and second motion will be seen very nicely, and that will be identified as a rare factor first motion. Therefore, if enough are seen it will be identified as an earthquake.

Representative BATES. Will you explain how you miss them?

Dr. BROWN. This question of how you might fail to detect something entirely?

Representative BATES. Yes.

Dr. BROWN. If the background noise is as big as I have shown, and instead of getting a seismic signal from the earthquake or explosion that is this big—let us say this is the noise—now say you get a seismic signal from a real event which is only this big.

If you add these two together you will see that this signal will go completely unnoticed in the background noise.

Representative HOSMER. That background noise also could be such things as trucks rolling along in the vicinity and other man-made disturbances of that nature?

Dr. BROWN. Seismologists try to locate their stations, sir, so that is rare. But it can happen. One can also, I believe, try to filter out certain frequencies of background noise, particularly the high frequencies. But what you cannot do very easily is filter out the actual motion of the earth in the same frequency range as seismic events.

These are called microseisms. They come from such things as pressure changes in the atmosphere, waves beating on the shore, and so on. For example, I have been told by my seismologist friends that in the Midwest one can detect on sensitive seismographs a storm in the Northwest coast, that is, in Washington and Oregon, just because of the increase of waves beating on the coast which send out seismic waves which travel thousands of miles.

Senator HICKENLOOPER. Dr. Brown, getting back to your illustration of your four station locations with respect to a hypothetical burst from an underground explosion, the intersecting circles or the crossing circles will produce, if you have four stations, theoretically a quadrangle of area, will they not?

Dr. BROWN. Yes, sir.

Senator HICKENLOOPER. Within that quadrangle someplace it is reasonable to assume that an explosion or a suspected incident may have occurred?

Dr. BROWN. Yes, sir.

Senator HICKENLOOPER. Do you have any formula by which you can give us the size, if it is three stations the triangle of error, or if it is four stations the quadrangle of error, and so on?

Dr. BROWN. If there are three or four stations then that area is likely to be quite large. It is likely to be perhaps 400 square miles.

Senator HICKENLOOPER. In other words, from your seismographic stations—and I presume it will vary somewhat with the distance of detecting stations from the point of suspected incident——

Dr. BROWN. It varies slightly with that. It varies more with the number of stations and with the knowledge of the velocity of seismic waves in different parts of the earth's crust, which are different in different parts of the earth's crust.

Senator HICKENLOOPER. In other words, that area which could be described, I think, as an unknown area, would be roughly in the neighborhood of 400 square miles?

Dr. BROWN. It would be if there were only three or four stations which detected the event. If there were 10 or 12 surrounding the source and if the velocity of the waves were known very well, which is not in most places, then you might get it down to the order of 100 square miles.

Senator HICKENLOOPER. That would be 10 miles on the side?

Dr. BROWN. Yes, sir.

Senator HICKENLOOPER. That is a pretty big area.

Dr. BROWN. Yes, sir.

Senator HICKENLOOPER. Even 4 or 5 miles on a side is a pretty big area.

Dr. BROWN. There can be lots of things in such an area.

Senator HICKENLOOPER. Would you have any comment on the difficulty of locating the exact spot in that area of probable error against a determined effort at concealment?

Dr. BROWN. I think it could be made very difficult, sir.

Representative HOLIFIELD. We have a session of the hearings on the decoupling which is on this very point.

Senator HICKENLOOPER. I am not talking about decoupling necessarily. I am talking about finding the site.

Dr. BROWN. Onsite inspection is a large problem all on its own. I notice there are several speakers who will talk about that.

Senator HICKENLOOPER. We will get to that later, then.

Dr. BROWN. As I have said, an explosion should produce an initial outward motion in all directions which should be seen provided that the seismic signal is large enough. This is theoretically a rather complicated problem which has not been sufficiently investigated.

However, for practical purposes it is probably correct to assume that, in fact, there is a distinctive initial outward motion from an explosion providing that one is close enough. The second motion in the opposite direction is larger and, therefore, if the first motion is small enough to be lost in the natural background of microseismic noise due to atmospheric disturbances, waves beating on the shore and so forth, the first motion will be lost.

The second motion which is downward will then appear to be the first and, under these circumstances, the conclusion will be drawn that the seismic event had sent out an initial rarefaction and initial rarefactions are indicative of earthquakes.

The Conference of Experts made estimates on the size of the noise to be expected with the instrumentation which they recommended. On the basis of this, they drew conclusions about the size of an earthquake in terms of the nuclear explosion with Rainier coupling which produces the same size signal, for which enough first motions would be detected by the seismic network above the noise background to enable the earthquake to be identified as such.

I will not go into any more detail on this, since the conclusions turned out to be wrong. I will just say what they were. They were that 90 percent of earthquakes which produce seismic signals equal to or greater than that produced by 5 kilotons nuclear explosion with Rainier coupling would be identifiable as earthquakes on the basis of their first motion.

Representative HOLIFIELD. When you speak of "Rainier coupling" what do you mean by that term?

Dr. BROWN. I mean the same fraction of energy roughly going into seismic waves detected at a distance—that is, seismic waves of a few seconds period—as was the case for Rainier.

Representative HOLIFIELD. In the same type of soil?

Dr. BROWN. It is in the same type of soil at the same depth; yes, sir; and closely coupled. That is, without building a big hole.

Representative HOLIFIELD. That was a small tunnel, about 700 feet underground?

Dr. BROWN. I think it was about 900 feet below the top surface, about 800 feet from the nearest surface, which was the side of the mountain.

Below 5 kiloton equivalent the identifiability falls off very rapidly with energy and at 1 kiloton equivalent probably only a few percent of continental earthquakes could be identified.

At the time of the Conference of Experts there was pretty general agreement on the magnitude scale and equivalence of nuclear explosions to earthquakes of various magnitudes. The seismicity of the earth was also generally agreed on, both the Eastern and Western scientists using Gutenberg's book on the seismicity of the earth.

As a result, it was believed that about 2,000 continental earthquakes per year of magnitude equivalent to or greater than that of a 5 kiloton underground nuclear explosion with a Rainier coupling took place every year. The statement that 90 or more percent of earthquakes above 5 kilotons could be identified as such by the first motions grew out of the assumption that two rarefactions were enough to identify as an earthquake.

On this basis, the United States calculated that there would then be about 100 earthquakes a year above 5 kiloton Rainier equivalent which would not be identified as such. The Soviets announced that the number was 20, on the basis of no particular calculation that we could see. Both numbers went into the final report in the statement to the effect that the number was estimated at between 20 and 100 on continents. That was the only estimate about numbers that did go into the report.

Senator GORE. You mean that in the final report you estimated that the number would be from 20 to 100?

Dr. BROWN. Yes, sir.

Senator GORE. Did the report explain that the Soviets estimated 20 and you estimated 100?

Dr. BROWN. No, sir; it did not do that.

Senator GORE. Then it was not clear.

Dr. BROWN. No, sir; it was not clear. As I said, not very much of the background went into the report. We came up with a number of 100 or so and put that in. The Soviets came up with a number of 20. We did feel that this number was uncertain by a large factor, and to some extent this may justify the inclusion of both numbers.

Senator GORE. I had read that and this is the first time I have been advised that this was not a composite judgment; that you put your judgment in and the statement of the Russians was put in and it is somewhere in between the two.

Dr. BROWN. Yes, sir. Let me see whether I can find the exact statement in the report. If I remember correctly, what it says is that it has been estimated as 20 to 100. It says:

> It has been estimated on the basis of existing data that the number of earthquakes which will be undistinguishable on the basis of their seismic signals——

Senator GORE. You are telling us, then, that on the basis of data, the American experts estimated 100, but that the Russian experts, without submitting any data, suggested the figure of 20?

Dr. BROWN. That is correct. As it later turned out, the subsequent evaluation of the experts' system on the basis of the Hardtack data gave numbers which were far outside that range anyway in the upward direction.

Senator GORE. So neither was correct?

Dr. BROWN. That is so. I think it was probably felt at that time that there was an uncertainty of a factor of a few. The subsequent experiments at Hardtack proved that there was an underestimate of the error. On the basis of the Hardtack results alone, one would probably expect the unidentified number to be far more than 100.

Senator GORE. About how many?

Dr. BROWN. I think this may come out in subsequent testimony. My recollection is that it is somewhere in the neighborhood of 1,000 or 1,500. Since then the system has been modified a good deal and the number has come down again.

Senator GORE. Thank you.

Representative HOLIFIELD. At this point, for the record, let us explain that a kiloton is equivalent to 1,000 tons of TNT.

Dr. BROWN. Yes, sir.

Representative HOLIFIELD. And that the generally accepted figure the Hiroshima explosion was around 19 kilotons.

Dr. BROWN. Yes, sir.

Representative HOLIFIELD. That puts it into some sort of perspective, the 5 kiloton that you speak of, and the 1 kiloton.

Dr. BROWN. Yes, sir. I am sure later we will hear of the effects of the explosions of 20 kilotons or more under certain conditions.

Representative HOLIFIELD. The committee is familiar with these terms. I merely inject that into the record for the layman who reads it and wonders what a kiloton or megaton is.

Dr. BROWN. Yes, sir.

For 1 kiloton, the number of continental earthquakes per year was estimated to be about 5,000. Of these only a few percent could be identified by the first motion criterion. This left many thousands throughout the world which could then be suspected of being nuclear explosions of between 1 and 5 kilotons—with Rainier coupling—and

about 100 which could be suspected of being nuclear explosions of 5 kilotons or more.

In general, I should point out that about 10 percent of the continental earthquakes take place in the Soviet Union, and about an equal fraction in the United States.

All this made the subject of onsite inspection a rather important one. The Conference spent several days considering this subject.

Because of geography, with most earthquakes taking place near national boundaries or on coastlines and in mountains, it is unlikely——

Senator HICKENLOOPER. Is the balance of the statement of Dr. Brown going into the record?

Representative HOLIFIELD. Yes, sir.

Senator HICKENLOOPER. I noticed the elimination of two sentences at this point which I wondered if he were excluding from his statement. One is the statement:

> The seismic network might be expected to locate the event within a couple of hundred kilometers if travel times were very well known and if the stations surrounded the event.

The other sentence following:

> One should point out that these conditions are met almost nowhere in the world now.

I do not know whether you meant to leave those two statements out of here.

Dr. BROWN. No, sir. I thought I perhaps covered some of them in answer to the questions that you put to me and Senator Gore put to me.

Senator HICKENLOOPER. I think you did. In my opinion they are very important statements.

Dr. BROWN. Yes, sir.

Representative HOLIFIELD. I agree with Senator Hickenlooper that you carefully prepared this statement and that we will not omit any part of your statement, but feel free to interpolate any additional explanatory statements as you wish as you go along.

Dr. BROWN. Thank you very much.

Because of geography, with most earthquakes taking place near national boundaries (because they take place in mountains), or on coastlines, it is unlikely that the Geneva network can ever locate events within so small an area. Given that degree of location, the Conference considered what methods could be used to further localize the event and to prove a nuclear explosion if one had occurred.

Since the radioactivity and heat can be contained within a few hundred feet or less of the site of a nuclear explosion, these can only be used to identify it when it has been localized within a very small region, and by "site" I mean underground, because they are contained within a few hundred feet deep underground.

Various geophysical methods have been investigated and it was found that most of them worked rather poorly. No radioactivity will escape. The anomalies in the acceleration of gravity or in magnetic or electromagnetic properties would be negligible in comparison with the naturally occurring variation, making these methods useless for further localization.

Seismic profiling seemed to be perhaps more hopeful, but could not be carried out in any event over the very large area within which the seismic stations would localize the event. Furthermore, certain conditions such as a high velocity cap rock might make it not terribly useful.

Suppose, then, that one were inspecting onsite an event which had actually been a nuclear explosion. What would be the probability that one would be able to localize it sufficiently so that drilling would allow it to be identified as such?

The Conference of Experts was unable to answer that question, and that is not surprising. Though further studies have taken place since then, the question can still not be answered today. If one reads what the Conference of Experts had to say about this question in its conclusions on the capability of the system, one finds that the statement that there would always be some probability of onsite inspection establishing a violation if one had occurred.

This is equivalent to saying that the probability is not zero. However, and this is also implied in the same paragraph by the statement that it might be very difficult for the system to catch a violator, the probability might be made very small by careful precautions taken by the violator.

Certainly the probability of success of onsite inspection at the site of an actual concealed explosion would not be 100 percent if the violator were to take any precautions at all. Whether very small means a half, a tenth, a hundredth, or a thousandth, I do not think that anyone could say at the time of the Conference of Experts, nor can they say now.

I should reemphasize that the probability of which I have just been speaking is that of success in onsite inspection only. It does not include any degradation produced by inspecting only a fraction of unidentified events, nor does it include the possibility of decoupling so that a nuclear explosion would not be detected at all, and thus could not be inspected.

Senator GORE. Does it include an arbitrarily and politically determined quota of inspections for an entire national area?

Dr. BROWN. No, sir. It omits the degradation produced by inspecting only a fraction of unidentified events. A politically determined quota or even a technically determined quota would be only a fraction of the naturally occurring unidentified events.

Senator GORE. In other words, the optimum possibility of detection would come only with full freedom to inspect according to the scientific and physical evidence.

Dr. BROWN. Yes, sir; that would be the optimum probability of detection, but it still might be very much less than one if someone had carefully tried to hide the explosion.

Perhaps I may digress a moment to say something about how the experts' system came to have 180 stations. If one reads the report of the Conference of Experts, it will be evident that the discussions of the capability of the various methods are all written in terms of 1 kiloton explosions for the methods that were first considered.

That is, the acoustic method is evaluated in terms of what it will do for 1 kiloton explosions; the radioactive debris method is written as regards the ability of a station to detect 1 kiloton and so forth. When the seismic methods of detection were discussed it became clear

that the identification of most earthquakes would be possible only with a very large number of stations, if one were to consider earthquakes whose seismic signals were equivalent in amplitude to those of an underground nuclear explosion underground, with Rainier coupling, of 1 kiloton.

One cannot identify explosions as such from their seismic signals without the additional action of onsite inspection. One can, however, hope to eliminate a large fraction of the events by identifying them as earthquakes. A natural number to take is 90 percent. This does not mean the system has 90 percent capability of identifying explosions of a particular yield, but that it can identify 90 percent of the earthquakes of equivalent size.

To do this for 90 percent of the earthquakes of 1 kiloton equivalent or more required, it turned out, something like 650 stations worldwide. However, 650 stations were something that the Eastern delegation would not accept. The Western delegation then made the proposal that the number of stations could be adjusted depending upon what capability or threshold one wanted to give the system.

If one wanted to be able to identify 90 percent of earthquakes or 5 kilotons equivalent with Rainier coupling, the spacing of stations could be increased. This corresponds to the 180-station network finally recommended by the Geneva Conference of Experts.

Senator GORE. With respect to this number 180, was this an agreed number recommended by the experts, or did it follow the pattern of the 20 to 100 to which you formerly referred?

Dr. BROWN. In this case I would say not, sir. I think the agreement was made to consider the 180-station system.

Or rather, we evaluated the 180-station system. Then we set forth its capabilities. Then there came an argument about whether it really took 180 stations to do this. The Soviets maintained it did not.

Senator GORE. You say earlier here that to identify 90 percent of the earthquakes of 1 kiloton equivalent, it turned out something like 650 stations were required in Russia.

Dr. BROWN. That is correct for 1 kiloton equivalent.

Senator GORE. How does the 5 kiloton compare with the 4.75 seismic signal suggested in President Eisenhower's proposal of February 11?

Dr. BROWN. With Rainier coupling; 4.75 corresponds fairly close to 20 kilotons.

Senator GORE. Are you going to have somebody on decoupling later?

Representative HOLIFIELD. Yes.

Senator GORE. I will desist. Excuse me, Doctor.

Dr. BROWN. The Western delegation felt that there was a very large number of possible systems of whose capability it was prepared to estimate, with various capabilities for identifying earthquakes at 5 kilotons, or 1 kiloton, or wherever.

However, having finally agreed to discuss the capabilities of 180-station system, the Soviets refused to consider any other. The report of the Conference of Experts is, therefore, cast in terms of 180-station network although at that time and subsequently there was an evaluation of various systems.

Roughly, one could describe the experts' system in terms of the capability it was believed to have at the time of the experts' conference

as a 5 kiloton system so far as underground explosions were concerned. Between 5 kilotons and 1 kiloton, its capability fell off very rapidly, being very near zero at 1 kiloton, at which only a fraction of earthquakes were even detected.

The report of the Conference of Experts makes it plain that there is a threshold below which the system has very little capability. Subsequent developments have indicted that some capabilities of the system recommended by the Conference of Experts are very much poorer than was believed at that time. The threshold may be much larger.

If one expresses the capability of the system in terms of identifying earthquakes of a given magnitude, then the capability of the system is moderately worse than believed at that time because of the degradation of the first-motion criterion.

If one thinks of the capability of the system in terms of its ability to detect and identify underground explosions and explosions in space, then later data show its capability is very much worse than was thought at that time, because of the possibility of great decreases in coupling which were not then appreciated for underground shots, and the possibility of shielding of explosions in space.

If I may say a final word about the Conference of Experts and its results, I would like to note that for most or all of the difficulties which have since come up with regard to the capability of the detection-identification system, one can go back to the report of the Conference and point to a passage in which at least some recognition was taken of them.

One can say, looking back, that perhaps not enough attention was paid to them, particularly in such items as the possibility of very large decoupling, the difficulties of onsite inspection and the possibilities of shielding explosions in space.

No specific method of decoupling had at that time been demonstrated. Perhaps it could thus not have been expected to get very much attention. Since a specific method has been demonstrated, I think now one should consider not only that method, but all the other possible methods of decoupling.

The Conference of Experts' report can logically be criticized for not taking adequate account of the self-evident fact that a violator of a treaty or a moratorium on nuclear weapons tests must be expected to take full advantage of whatever methods he can find to reduce the probability of detection.

It is, therefore, rather misleading to consider only the probability of detection of an unconcealed shot. We must not ignore the very difficult problem of detection and identification of nuclear explosions which are intended to go unidentified, and for which every possible precaution would be taken to keep them undetected or unidentified.

Since the time of the Conference of Experts of course very much more realistic consideration has been given to this question.

Thank you.

Representative HOLIFIELD. Are there any questions?

Senator GORE. Doctor, you were a member of the Conference of Experts?

Dr. BROWN. Yes, sir. I was an advisor to the Western delegation.

Senatore GORE. You were in Geneva at the time the Conference began?

Dr. BROWN. Yes, sir.

Senator GORE. I saw your work there and found it, according to my estimates, entirely praiseworthy. I congratulate you on it.

Dr. BROWN. Thank you very much, sir.

Representative HOLIFIELD. Are there any further questions?

Representative HOSMER. In your discussion of estimated capabilities of detecting violations in space, your discussion was very brief. Is that similar to the problem of underground detection where some new knowledge has introduced new factors and additional difficulties?

Dr. BROWN. Yes, sir. I believe this will be covered at length later. In fact, quite soon.

Representative HOSMER. Then I want to get this clear. To actually pin down cheating on the test ban, there are at least two things required: First, the detection of a suspicious event which in and of itself is in no way a matter of certainty; is that correct?

Dr. BROWN. That is correct; certainly at some level.

Representative HOSMER. Then, secondly, having found the suspicious event and located it within some geographical range, an inspection must follow in order to determine what, if anything, happened.

Dr. BROWN. In the case of underground explosions. In the case of explosions in space that possibility is not open. That makes absolute identification perhaps even more difficult.

Representative HOSMER. That is right. Again in connection with underground explosions, the actual inspection business is less of a scientific matter than it is a detective matter; is that correct?

Dr. BROWN. In my opinion that is correct, sir.

Representative HOSMER. The actual inspection guarantees no certainty that an instance of cheating would actually be discovered?

Dr. BROWN. That is correct. The way to put it, perhaps, is that if you have an onsite inspection on the actual site of the explosion it is not certain that it will go undetected. But it is certainly not certain that it will not.

Representative HOSMER. In addition to all your natural factors, there can be introduced, both as regards to detection and as regards to inspection, matters by humans to throw the investigators off the trail?

Dr. BROWN. Yes, sir. This is certainly so below some yield. I think there are yields large enough so that they will almost certainly be detected. In the atmosphere, of course, that yield is rather low. In space, or underground, that yield may be very large.

Representative HOSMER. Thank you.

Representative PRICE. Mr. Chairman, may I ask a few questions?

Representative HOLIFIELD. Mr. Price.

Representative PRICE. Dr. Brown, on an onsite inspection, are there conclusive ways of proving if it were actually an earthquake?

Dr. BROWN. No one, so far as I know, has any good way of proving this.

Representative PRICE. What are the problems involved?

Dr. BROWN. I think that perhaps one could prove that something like the San Francisco earthquake were an earthquake because there was motion along a very, very long line. If there is motion along tens of miles, then you probably believe it is an earthquake. I think that the superficial indications, ground disturbances over a comparatively local area, probably do not serve to distinguish between an earthquake and an explosion. As most people are thinking about

the problem of onsite inspection now, and again the experts on this will come later, I think that most of them believe the way to prove there is an earthquake is to fail to find a nuclear explosion. I don't know that that is a very good way to do it.

Representative HOLIFIELD. How about the detection of residual radioactivity?

Dr. BROWN. That is the conclusive proof of a nuclear explosion if you can find it. To find it, of course, you have to get right into a very small region, a few hundred feet in diameter, and perhaps thousands of feet underground out of this 100- or several-hundred-mile area.

Representative HOLIFIELD. In other words, it would involve drilling down to the point of the explosion. Say, if the explosion were 900 feet deep, you would dig down at least toward that explosion?

Dr. BROWN. Yes, sir.

Representative HOLIFIELD. Your drilling would have to be pretty accurately located in order to be sure that you would drill into the radioactive area.

Dr. BROWN. Yes, sir; it would have to be very accurately located.

Representative HOLIFIELD. What was the experience on Ranier on this particular point when they started to drill in? They knew exactly where it was.

Dr. BROWN. It took something like 3 months, if I remember correctly, to arrive at the radioactivity. This was partly due to losing the drill down the drill hole and so on. I think one should also point out that it perhaps is not typical because this was the first time that an underground nuclear explosion, as far as we knew, had taken place. We did not know whether or not there was a big bubble of very hot and radioactive gas trapped underground. We didn't want to drill in and have it all come out in our faces. As it finally turned out, we gave up drilling from the top down. We went into the tunnel which led to the room where the explosion had taken place and finally drilled in sideways from a distance of only 200 feet away. This, as I say, was several months after the explosion.

Senator GORE. You mean you were never able to penetrate the cavity from surface boring?

Dr. BROWN. We finally did penetrate the cavity from the top but I don't think we recovered anything that way. We finally recovered from the side.

Senator GORE. Did you finally penetrate the cavity from perpendicular boring?

Dr. BROWN. Yes, sir.

Senator GORE. How long did that require?

Dr. BROWN. Again, I am not quite sure. There are people here who can correct me if I am wrong. What I remember is that we drilled down finally all the way into the cavity just to make sure there was no bubble of high pressure. But we did not recover a sample from within the cavity from above. Our recovery and identification of the radioactivity finally came from the side after we had penetrated from the top but not recovered from the top.

Senator GORE. Why was it the perpendicular boring into the cavity did not reveal radioactive samples?

Dr. BROWN. It did reveal a radioactive sample quite high up, actually. But as I say, we had difficulty getting good samples and finally we drove from the side.

Senator GORE. You did detect radioactivity close to the surface from your perpendicular boring?

Dr. BROWN. We detected radioactivity about halfway down. This was radioactivity which came from gases that had moved up several hundred feet and then condensed. This did not reveal an active region to a Geiger counter that was put into the hole. What we had to do was to extract gas from the hole, process it, run it through chemical processing, and make very careful measurements of that gas sample to see that there was some radioactivity in that region.

Senator GORE. The earth itself did not register on the Geiger counter?

Dr. BROWN. That is correct.

Senator GORE. Mr. Chairman, this is the first time I have heard that radioactivity was detected some hundreds of feet above the radioactive cavity. I had heretofore understood that the danger to the earth's water table was minimized or eliminated by the conversion of the sand and the rock and the earth to glass and the encasement of radioactivity within the glass. You tell us now that there was radioactivity hundreds of feet above the cavity.

Dr. BROWN. There was some, sir. It was a very small fraction of a percent. Let me say roughly what the radioactivity situation was. There was a spherical shell the order of 110 feet in diameter which contained well over 90 percent of the radioactivity. Within this shell there was some more sand which contained some more radioactivity, almost all the rest. For a distance of perhaps 200 feet or 300 feet, which is comparable with the size of the cavity in the vertical direction, there was the order of a couple of tenths of a percent of the radioactivity. Though it had risen it was still about 500 feet below the surface of the earth. Water samples introduced into this region have shown that there is no further diffusion of more than perhaps 10 feet or so beyond the edges of this cavity in a radial or outward direction. So it is confined in a volume which is not very much bigger than this sphere. It is perhaps two or three times as big in the upward direction. But it stops far below the surface of the earth. Our experiments have shown that there is no ground water carriage of radioactivity more than a few feet in the outward direction.

Senator GORE. So by this further exploration you confirmed the information previously given this committee that the underground table will not be contaminated with properly conducted underground explosions?

Dr. BROWN. Yes, sir.

Senator GORE. I have one further question about this radioactivity some hundreds of feet above the point of explosion. Was this amount of radioactivity sufficient to indicate to an inspection team the occurrence of a nuclear explosion at some point deeper beneath the earth, or was it of a degree which could have existed in the earth from natural causes?

Dr. BROWN. It depends on how careful a measurement they would have made. It is my personal opinion that if they had finally gotten around to drilling they would by this time be so suspicious that they would be making very, very careful measurements indeed, and therefore probably if it didn't constitute identification, it would certainly lead to ultimate identification. But that is based on my opinion of how suspicious they would have to be before they got around to drill-

ing. If this is just an ordinary drilling team sent out to find oil, this would not identify anything to them. But I think an inspection team would be very, very suspicious—would have to be very, very suspicious that a nuclear explosion had taken place—before they drilled.

Senator GORE. Thank you, Mr. Chairman.

Representative HOLIFIELD. Mr. Bates.

Representative BATES. Dr. Brown, you were with the Russian scientists for several days over there and you talked to them either directly or through an interpreter. You read their statements and studied their reactions. I have noticed here that there is a difference of opinion. I wonder if for the purpose of the record that they in true scientific fashion tried to get at the bottom of these very complex matters or whether they just unduly rejected all the attempts to find satisfactory answers to these problems. Can you pass a judgment on that?

Dr. BROWN. Yes, sir; I think it is a very tentative one. It is not really very well informed even though I have spent several months talking to some of them. I think this is by now very much a political negotiation, although it still must have some technical basis. Under these circumstances I am not surprised, although I can't say I am at all pleased, that the Soviet scientists are very strongly politically motivated. Our own delegations, of course, have some political motivation, too, in that they represent the United States and therefore they tend to represent to some extent U.S. policy, which is that agreements on arms limitations have to be controlled. We therefore tend to be quite careful in our evaluation of possible measures of control. We never distort the technical facts to fit political opinions, however. The Soviets tend to take quite the opposite view. I think they depart far more from purely scientific considerations and motivations than do our scientists. This puts us in a somewhat awkward position. It throws the burden of the work on us. We are trying to prove or trying to understand just how well certain methods can control certain obligations or agreements. The Soviets have an easier time of it because they can say it is simple and everybody knows it is simple, and that is the end of it. They do, I think, when we present something analyze it with some scientific care. They sometimes come back with some rather good scientific questions which further clarify the situation. But on a number of occasions, I have seen them accept something in private scientifically, or at least appear to accept it scientifically, and then come back for the record with quite a different attitude.

Representative BATES. Without any technical explanation?

Dr. BROWN. There is always a technical explanation. Sometimes it holds almost no water. Sometimes it is quite fallacious. Other times there is some basis to what they say.

Representative BATES. I understand this point will be covered later on in the hearings.

Representative HOLIFIELD. Mr. Westland.

Representative WESTLAND. Doctor, as I understand it you have about 5,000 suspected earthquakes a year in Soviet Russia. Is that correct?

Dr. BROWN. 5,000 earthquakes equivalent to 1 kiloton with Ranier coupling in the world of which 10 percent are in the Soviet Union which constitutes about 500.

Representative WESTLAND. I understand that there were 5,000 in the world.

Dr. BROWN. That is right.

Representative WESTLAND. And about 500 in Soviet Russia?

Dr. BROWN. That is right.

Representative WESTLAND. Then how many would you expect in this country?

Dr. BROWN. About an equal number.

Representative WESTLAND. About an equal amount?

Dr. BROWN. Yes, sir.

Representative WESTLAND. So then if you were to have these 20 inspections, you would have about 25 to 1 odds.

Dr. BROWN. You would have 1 chance in 25 of inspecting a nuclear explosion if somebody had been foolish enough to set off a nuclear explosion with a seismic signal that was easily detected. I think if somebody is going to carry out a nuclear explosion he is going to take rather extreme precautions. It would only be an accident, I think, if it were detected. If it were detected then there would be according to this arithmetic 20 out of 500 or 1 out of 25 chances that you would have an onsite inspection, not allowing for any additional selection you might hope to be able to make either on the basis of the seismic signals or on the basis of likelihood figured some other way. This would still leave the question of whether you would find the event even if you inspected it.

Representative WESTLAND. But the odds would be 25 to 1 against you?

Dr. BROWN. To start with, that is correct.

Representative HOLIFIELD. Senator Hickenlooper.

Senator HICKENLOOPER. Dr. Brown, on page 8 of your prepared statement at the bottom of the page, you pose a question and then answer it on page 9. It starts at the beginning of the paragraph at the bottom of page 8 with the word "suppose."[2]

Dr. BROWN. Yes, sir.

Senator HICKENLOOPER (reading):

Suppose, then, one were inspecting onsite an event which had actually been a nuclear explosion, what would be the probability that one would be able to localize it sufficiently so that drilling would allow it to be identified as such.

That is the question.

Then you say—

The Conference of Experts was unable to answer that question and that is not surprising. Though further studies have taken place since then, the question can still not be answered today. If one reads what the Conference of Experts had to say about this question in its conclusions on the capability of the system, one finds that the statement that there would always be some probability of onsite inspection establishing a violation if one had occurred.

Then you make this comment:

This is equivalent to saying that the probability is not zero.

Would you say the converse is true, that the probability of establishing a violation is almost zero?

Dr. BROWN. It can be made very small. This is a subject which is virtually impossible to express in quantitative terms. I have tried. Lots of other people have tried. But no one believes anybody else's estimate.

[2] See Dr. Brown's prepared statement, pp. 33-34.

Senator HICKENLOOPER. You have made the statement that this is equivalent to saying that the probability is not zero.

Dr. BROWN. Not exactly zero.

Senator HICKENLOOPER. I am just wondering if it is also your opinion that it approaches zero. As Senator Aiken suggests, what is its half life.

Dr. BROWN. This is a matter of opinion. I have an opinion. I can't even call it a technical opinion because this is not completely a technical subject. I think it can be made very close to zero.

Senator HICKENLOOPER. My own personal interpretation of your statement would be somewhat along that line.

You say on page 11 in the first paragraph, about the middle of the paragraph, that the report of the Conference of Experts makes it plain that there is a threshhold below which the system has very little capability.

Did the Conference of Experts come to a conclusion as to how many kilotons that threshhold amounts to?

Dr. BROWN. It never talks about anything less than kiloton with Rainier coupling for underground explosions. It says that you can detect most earthquakes of 1 kiloton equivalent to seismic regions. It also says that the capability of the control system to identify underground nuclear explosions of 1 to 5 kiloton yield—again this is always with Rainier coupling—depends on the small fraction of earthquakes that can be identified on the basis of data obtained from the control posts, the fraction of earthquakes that can be identified with the aid of supplementary data obtained from existing seismic stations. Incidentally, the existing seismic stations have more or less disappeared from consideration, at least in the conference on discontinuance of nuclear tests. I think that is because people are now, including the Soviets, agreed that existing seismic stations are very poor compared to the seismic stations we are talking about and won't increase the capability very much. Then it says that the fraction of events still left unidentified which would be expected to be nuclear explosions and for which the international control organ carries out inspection in accordance with item 6 that is onsite inspection. If one takes these together and asks what it means, again everybody interprets as they like. I think what that says is that below 1 kiloton, even the experts' report says there was no capability, with Rainier coupling. It seemed to imply there was a fairly good capability for 5 kilotons. That has been modified by subsequent events and discoveries. Between those two yields the capability fell off pretty rapidly.

Senator HICKENLOOPER. Dr. Brown, if two people get into an agreement and as a result if it is an important agreement they write a contract in which they specify the details of that agreement. If these two people entered into the contract exactly alike and would interpret it exactly alike there would be no occasion for a written agreement. The written agreement is only to specify the terms and conditions so that there can be no reasonable question about what the performance shall be. Now we are talking here not about nuclear underground explosions that are announced to the world and open to full inspection and publicity. We are talking about a situation where the security of the system depends upon ability to locate and prove whether or not an atomic explosion has occurred when there is a determined effort on the part of some country to conceal that explosion and cover it up. You stated a month ago that normally it would be from 100 to 400

square miles of probable error from seismographic locaters. On the Rainier shot you knew probably within 1 to 2 feet exactly the location of this explosion. You knew that an explosion had occurred. Suppose you had gone out in an area of 100 to 400 square miles without knowing for sure whether an atomic explosion had occurred, or knowing within that probable error as to where it had occurred, do you think you could have ever discovered the Rainier shot with any probability at all?

Dr. BROWN. The specific Rainier shot probably, I think, would have been quite well located by the network of roads and by the fact that there were people there.

Senator HICKENLOOPER. I want to make clear that I am talking about a determined effort at concealment.

Dr. BROWN. Rainier was certainly not that. So we have essentially nothing to base judgment upon.

Senator HICKENLOOPER. Would you not say that the probabilities of locating almost any size of underground atomic explosion where there was a determined effort at concealment of the location, with all the forces that could be brought to bear for concealment, moving out of personnel, the concealment of any access roads, sufficient depth to the shot, putting it way down in the earth, that the possibility under any circumstance of locating the spot of that explosion or the proof of whether or not an explosion occurred under any circumstance with presently available equipment or equipment in the reasonable future is practically nil?

Dr. BROWN. I think it is very small. For practical purposes, it may be quite close to zero. It depends so much on the circumstances. It depends a great deal on how well someone is able to anticipate what effects may be, although he could of course start off with small explosions which nobody could find and then scale up. It depends on the degree of access. It depends on what one is able to find out in other ways. One may be able to know something about activities in various areas which would enable one to localize things. I think that is something that one might hope to depend upon. I myself would prefer not to depend on it very much. But then I think—here again it is a personal opinion—it is fairly important to have more than a rather slim chance of catching someone. The fact that he is anxious for you not to catch him is likely to result in a very small chance of your catching him. I think one must weigh here the capabilities, the technical capabilities of this system with whatever political considerations have to be put in on the desirability of such a treaty. I am not here trying to put in such considerations.

Senator HICKENLOOPER. Of course, the more accurate the detection system the less likelihood of violation. There is no question about that.

Dr. BROWN. As you make the system have more and more capability, you force someone to more and more effort to hide it. I think if he makes enough of an effort he probably can hide it. If you make that effort great enough perhaps he won't think it is worthwhile. That depends on another question, and that is, how much can you do in the development of nuclear weapons by concealed testing and how important it is. Here again I have an opinion but it is only partly a technical opinion.

Senator HICKENLOOPER. Would you go further to say that under any system which we now have or which can be put into effect in the

foreseeable future that the detection and identification of explosions of 1 kiloton or less are practically nil and no system could really detect and identify them against any reasonable effort of concealment by anyone who wanted to violate.

Dr. BROWN. There is always some level below which a violator can test with essentially complete impunity. I think if a violator is willing to go to a little trouble, 1 kiloton is well within that limit. It will be undetected and unidentified by any system which I have heard proposed.

Senator HICKENLOOPER. Thank you.

Representative HOLIFIELD. Senator Aiken.

Senator AIKEN. Are you satisfied that underground tests would have the same value for determining destructiveness of an atom bomb for military purpose as an atmospheric test would have, or is there some drawback to underground tests?

Dr. BROWN. I think perhaps there is some drawback but I don't think it is very large. I think the military effectiveness of nuclear explosions in the atmosphere has by now been determined many, many times in various tests in the atmosphere, and that problem is quite well covered by now. The underground test really would be aimed at something different. Those tests would be aimed at determining the energy yield and characteristics of a new design of explosion.

Senator AIKEN. Then for developing smaller size nuclear bombs, underground tests would be just as valuable as if they were held above ground.

Dr. BROWN. I would say in general, yes. There is one exception to this that I perhaps should point out, and that is, the effects in the upper atmosphere which may go to determine some military applications are not completely understood. Those could not easily be determined underground.

Senator AIKEN. Then if underground tests are just as efficient for determining the destructiveness of the nuclear bomb for purposes of war, how efficient would any agreement be which excluded underground tests?

Dr. BROWN. This goes to the question of what one can develop by further nuclear tests.

Senator AIKEN. It would not mean anything.

Dr. BROWN. I am willing to offer an opinion on that if you would like to hear it.

Senator AIKEN. Would the underground test have greater value for determining the peaceful use of nuclear explosions? Would it have advantages over tests held above ground?

Dr. BROWN. For nonmilitary use, most of the applications—in fact, perhaps almost all except for a few scientific applications—depend on either shallow subsurface or deep surface underground explosions. The effects of those are not as well determined as in the atmosphere.

Of course, there are very few military effects from deep underground explosions. For the purpose of determining the usefulness of nuclear explosives for nonmilitary purposes, underground explosions would be distinctly much more valuable than explosions in the atmosphere.

Senator AIKEN. In determining the value of nuclear explosions for extracting oil from shale, for instance, the underground test would be essential, would it not?

Dr. BROWN. Yes, sir. They are essential and they are all that is essential.

Senator AIKEN. Now, my elementary question: There is objection to testing above surface because of the fallout. What becomes of strontium 90 and other fallout in an underground test?

Dr. BROWN. If the proper medium is picked, a medium which contains silicon, for example, these fission products such as strontium 90 are confined within a few tens of feet of the region in which they are deposited by the explosion. That means altogether few hundreds of feet.

Even if ground water runs through such a region, we have found in the case of the Rainier explosion that over a period of several years—2 years at least—the fission products are not carried more than a few tens of feet away.

Senator AIKEN. That is all, Mr. Chairman.

Representative HOLIFIELD. The Chair will state that there will be other witnesses, of course, who will take different viewpoints from Dr. Brown, and that Dr. Brown will also be with us again and there will be panels in which some of these matters which Dr. Brown has been testifying on will be a matter of debate.

Senator GORE. Mr. Chairman, I would like to observe that Dr. Brown has attempted to confine his statements to technical and scientific opinion and he has not attempted to become a diplomat or a politician.

Representative HOLIFIELD. We hope that all the witnesses will do that. We hope also the members of the committee will observe the objectivity of the purpose of this hearing. The witnesses have been chosen because of their expert background. We feel we have a balance here of scientific experts who define these problems in different terms in order to have a balance for people to study both viewpoints.

We will have some people here who are not so pessimistic in regard to detection as Dr. Brown is, and they will be allowed to state their case with the same courtesy and the same amount of time as any witness will be granted by the committee.

So we will ask everyone to withhold their judgment on this matter until all of the returns are in.

Representative HOSMER. I just wonder, Dr. Brown, at the 1958 Geneva Conference did the Soviet scientific group contribute any information on the basis of actual underground tests conducted by them or was it all based on theory?

Dr. BROWN. Most of it was theoretical. They had a few seismograms from surface chemical explosions because they were explosions that vented into the atmosphere.*

Representative HOSMER. But no underground nuclear explosions?

Dr. BROWN. No completely contained underground nuclear explosions.

Representative HOSMER. In your opinion, would it be necessary if any kind of a detection system were established to run a few underground nuclear explosions in order to test it out?

Dr. BROWN. I think there are many things that can be determined about a system without underground nuclear explosions. I think

*The U.S.S.R. representatives at the 1958 Conference of Experts did not offer any data on underground nuclear explosions. In 1959, during the meetings of Technical Working Group II, the Soviet representatives stated that the U.S.S.R. had conducted no underground nuclear explosions.

there are some vital things which require nuclear explosions to find out. I think there are many other things which can best be determined most quickly and most cheaply and most accurately with underground nuclear explosions.

Representative HOSMER. Thank you.

Representative HOLIFIELD. The Chair will take this occasion to announce—I know the press will be interested in this—that today the Department of State has released the verbatim documents of the Geneva Conference on the Discontinuance of Nuclear Tests from the period October 31, 1958, to February 29, 1960. I might announce that in that group of transcripts that are now released to the press and public will be the very important release of the transcript of February 11 in which the U.S. delegates offered after 17 months of failure to obtain any definition of the so-called quota principle a formula which, in effect, did set up a tangible number of around 20 or 21, and that offer is contained in the February 11 release which the State Department is releasing verbatim.

The release was made pursuant to an agreement reached last month by the three negotiating parties, the United States, the United Kingdom, and the U.S.S.R. When the negotiations commenced on October 31, 1958, the three nations involved agreed to conduct the Conference sessions in private. However, in view of the long duration of the Conference, the three parties have decided to make available to the public the transcripts of past sessions through February 29, 1960. Transcripts and documents for sessions after this date will be released on a monthly basis, 1 month after the sessions take place.

Accordingly, release of the March verbatims and agreed documents will be made on May 1, 1960, and the April verbatims and documents on June 1, 1960. This will be followed for release of subsequent sessions.

The Chair regrets that the release of the March verbatims will be after the conclusion of these hearings. There are two verbatim transcripts, March 19 and 21, which constitute the Soviet reply to the United States and free world offer of February 11. The 19th verbatim has the first response of the Soviets and the 21st verbatim has a clarification of their response on the 19th. (See app. 2, p. 527, for the verbatim transcripts of February 11, March 19 and 21.)

Representative PRICE. Mr. Chairman, I would like to get some clarification from Dr. Brown as a result of the question by Senator Hickenlooper on your opinions on the possibility of certain types of detection.

Was your testimony based on the existing seismic equipment, or were you referring to the 180-station network, and do you take into consideration improvements in seismic equipment?

Dr. BROWN. I was talking about the capabilities of the 180-station network as believed to exist at that time. In other words, what people felt in 1958 to be the capabilities of that system. That does not take into account seismic improvements. It does not take into account decoupling or subsequent observations which may have shown that the system had less capability than thought, nor does it take into account that some of the equipment which was discussed at the time of the Conference of Experts and subsequently does not, in fact, exist nor did it exist then.

In other words, it was a hypothetical system with probably better equipment than had existed until that time or than exists now.

Representative PRICE. Thank you very much, Doctor.

Representative HOLIFIELD. For the record, Dr. Brown was the consultant of the first group.

Dr. BROWN. I was an adviser to the U.S. delegation to the Conference of Experts.

Representative HOLIFIELD. And the biography of Dr. Brown and all the other witnesses will be inserted in the proper place in the record of the hearings.

Thank you very much, Dr. Brown, for your testimony.

Dr. BROWN. Thank you, sir.

(Dr. Brown's formal statement follows:)

TECHNICAL CONCLUSIONS OF THE EXPERT'S CONFERENCE OF THE SUMMER OF 1958

Mr. Chairman, members of the committee, it is a great pleasure to appear before you. The purpose of this talk is to explain the matters which were considered by the Conference of Experts which met in Geneva the summer of 1958 and to outline the conclusions which that Conference reached. I will also say something about the uncertainties which were recognized to exist at the time and the matters which were then omitted from consideration for a variety of reasons.

The Conference of Experts considered a number of techniques for the detection and identification of nuclear explosions, attempting to evaluate them on the basis of what was then known. These methods include the air acoustic and hydroacoustic signals, the collection of radioactive debris from the atmosphere, the electromagnetic signal, the seismic waves from an explosion. They also include the detection of nuclear radiation by earth satellites and the measurement of ionospheric disturbances for the detection and identification of nuclear explosions high in the atmosphere or in deep space.

During the Conference of Experts it became apparent, and was no surprise to anyone, the Soviets were in favor of as small a number of stations and as minimal an accessibility by the control organization to the Soviet Union as they could get the Western delegation to agree to. Their action in this direction took the form of exaggeration of the capabilities of each of the methods for detection. By producing experimental evidence or more accurate theories than the Soviets, the Western delegation was able to get its evaluation of the individual capabilities agreed to by the Soviets, although the language was not always as clear as could have been hoped had the Soviets been operating on a purely technical basis. The Soviet delegation did finally agree to a number of points which are important as matters of principle. They did agree to dispatch special flights for the detection of possible radioactive clouds where there were acoustic or electromagnetic indications of possible nuclear explosions. It was also agreed that completely contained underground nuclear explosions, although they could be detected down to some yield by this system, could not be identified by seismic readings. Consequently the report indicated that for seismic events not identified as earthquakes by the control posts, inspection at the site of the event would be necessary in order to identify it either as an explosion or as an earthquake. No agreement was reached at that time on recommendations for the apparatus to be used for the detection of nuclear explosion in the outer atmosphere or in space, although certain statements were made about the capabilities of some of these methods.

The report of the Conference concluded that for nuclear weapons explosions of 1 kiloton or more in the atmosphere up to about 50 kilometers and for nuclear explosions in the oceans there would be a fairly high probability of identification on the basis of the remote signals, together in some cases with onsite inspection. This capability rests principally on the air-acoustic and radioactive-debris techniques for the explosions in the atmosphere, supplemented in the case of explosions on the surface by an onsite search for radioactivity. The electromagnetic signal, though useful as confirmatory evidence, can be greatly decreased by surrounding the explosion with tons of shielding material, and therefore its absence does not constitute evidence that the event is not a nuclear explosion.

There are areas over the oceans where thne et spacing is rather sparse and where atmospheric explosions of one or even several kilotons might often fail to be detected. However, as a general rule the statement on capability for these atmospheric explosions corresponded to a rather high probability of detection. Thus, for the acoustic method the spacing of 1,700 kilometers gave probabilities of 70–90 percent detection of the acoustic signal of a kiloton explosion at enough

stations to locate the event. In addition, the radioactivity present in the atmosphere or in the ocean would, with substantial probability, provide confirmation. This technique would enable the natural events, occurring with fair frequency (perhaps a hundred annually worldwide of 1 kiloton acoustic equivalent) which simulate the acoustic signals by nuclear explosions to be eliminated by a cursory onsite inspection. These events are principally meteors or volcano eruptions. It is thus apparent that for explosions in the atmosphere there would be a substantial probability (of the order of 50 percent) of detection and identification in the range of 1 or 2 kilotons even if elaborate attempts were made at concealment. I mention all this as background to indicate the kind of identification probability that was considered adequate for control in the case of atmospheric explosions.

In the case of explosions in space an estimate was made of the capabilities of the satellite system for the detection of nuclear explosions. It was estimated that unshielded nuclear explosions of 1 kiloton could be detected at a distance up to hundreds of thousands of kilometers by their gamma radiations. The exact range of detection was known to depend on the backgrounds of radiation which would be encountered by the satellites in the vicinity of the earth, about which not very much was known in 1958 except the existence, together with some very preliminary ideas of its nature, of the Van Allen Belt.

As to underground nuclear explosions, everything was normalized or calibrated to the coupling that is, the degree of energy transfer into seismic waves, observed in the Rainier explosion, which was the only underground nuclear explosion carried out before the Conference of Experts so far as we know. A footnote in the report of the Conference of Experts pointed out that smaller couplings than this might be possible. If so, then all the statements made about 5 kilotons in that report, for example, would correspond to yields of underground explosions equal to 5 kilotons times whatever the factor of reduction in coupling was. At the time of the Conference of Experts, it was believed that other media might have coupling less than Rainier or more than Rainier by a factor of a few depending on how the properties of medium compared with those of Nevada tuff in which the Rainier explosion took place. The thought that decouplings of a large factor, of the order of hundreds, might exist had occurred to many of us, but no clear understanding existed at that time about what the modification of the medium would be necessary in order to make this decoupling take place. In the absence of such understanding, that possibility of large decoupling was pretty generally ignored.

The Conference concluded that the system would be able to locate, in seismic regions where the separation of stations was a thousand kilometers, most earthquakes or underground explosions whose seismic signals was greater than or equal to that from a 1-kiloton nuclear explosion with Rainier coupling. In aseismic regions, where the spacing is rather larger—1,700 kilometers—one can calculate that the yield required for a good probability of location is about 5 kilotons equivalent yield. For location purposes one needs to be able to obtain a clear time of arrival from at least three or four seismic stations. Failing this criterion, one can perhaps obtain wiggles on one or more seismic graphs which are due to the seismic event (be it earthquake or explosion) but it cannot be said to be detected in any true sense if its position is unknown.

Now one cannot tell from seismic signals that an event is an underground explosion. It is therefore important to note that there are a very large number of seismic events annually which give signals equal in magnitude to that produced by a nuclear explosion of 1 kiloton or more with Rainier coupling. One then asks how many of the seismic signals produced by earthquakes can be identified as such by the system of seismic detection posts, so as to find out whether one can keep the system from being swamped by the number of events which could be nuclear explosions. The Conference of Experts tried to find a number of such possible distinguishing characteristics. About half of the earthquakes may be eliminated on the basis of their taking place underneath the ocean floor where the depth of the floor is too deep for nuclear explosions to be carried out in any foreseeable way. There are also a number of earthquakes on the continents which have too deep a focus for humans to have reached. However, these amount to only about 10 percent. The Conference of Experts adopted as a principal technique which the system would use to identify earthquakes as such the so-called criterion of first motion. It is generally considered that a large enough underground explosion should be characterized by a positive first motion at all seismic stations which detect it. In other words, an explosion should produce an initial outward motion in all directions which should be seen provided that the seismic signal is large enough. This is theoretically a rather complicated problem which has not been sufficiently investigated. However, for practical purposes it is

probably correct to assume that in fact there is a distinctive initial outward motion from an explosion providing that one is close enough. The second motion in the opposite direction is larger and therefore if the first motion is small enough to be lost in the natural background of microseismic noise, due to atmospheric disturbances, waves beating on the shore, and so forth, the first motion will be lost. The second motion which is downward will then appear to be the first and under these circumstances the conclusion will be drawn that the seismic event had sent out an initial rarefaction and initial rarefactions are indicative of earthquakes.

The Conference of Experts made estimates on the size of the noise to be expected with the instrumentation which they recommended. On the basis of this they drew conclusions about the size of an earthquake in terms of the nuclear explosion with Rainier coupling which produces the same size signal, for which enough first motions would be detected by the seismic network above the noise background to enable the earthquake to be identified as such. I won't go into any more detail on this since the conclusions turned out to be wrong; I'll just say what they were. They were that 90 percent of earthquakes which produce seismic signals equal to or greater than that produced by 5 kilotons nuclear explosion with Rainier coupling would be identifiable as earthquakes on the basis of their first motion. Below 5 kiloton equivalent the identifiability falls off very rapidly with energy and at 1 kiloton equivalent probably only a few percent of continental earthquakes could be identified.

At the time of the Conference of Experts there was pretty general agreement on the magnitude scale and equivalence of nuclear explosions to earthquakes of various magnitudes. The seismicity of the earth was also generally agreed on, both the Eastern and Western scientists using Guttenberg's book on the seismicity of the earth. As a result it was believed that about 2,000 continental earthquakes per year of magnitude equivalent to or greater than that of a 5 kiloton underground nuclear explosion with a Rainier took place every year. The statement that 90 or more percent of earthquakes above 5 kilotons could be identified as such by the first motions grew out of the assumption that two clear rarefactions were enough to identify as an earthquake. On this basis the United States calculated that there would then be about a hundred earthquakes a year above 5 kiloton equivalent which would not be identified as such. The Soviets announced that the number was 20, on the basis of no particular calculation that we could see. Both numbers went into the final report in the statement to the effect that the number was estimated at between 20 and 100 on continents.

For 1 kiloton the number of continental earthquakes per year was estimated to be about 5,000. Of these only a few percent could be identified by the first motion criterion. This left many thousands throughout the world which could then be suspected of being nuclear explosions of between 1 and 5 kilotons (with Rainier coupling) and about a hundred which could be suspected of being nuclear explosions of 5 kilotons or more. In general I should point out that about 10 percent of the continental earthquakes take place in the Soviet Union, and about an equal fraction in the United States.

All this made the subject of onsite inspection a rather important one. The Conference spent several days considering this subject. The seismic network might be expected to locate the event within a couple of hundred kilometers if travel times were very well known and if the stations surrounded the event. One should point out that these conditions are met almost nowhere in the world now. Because of geography, with most earthquakes taking place near national boundaries or on coastlines, it is unlikely that the Geneva network can ever locate events within so small an area. Given that degree of location the Conference considered what methods could be used to further localize the event and to prove a nuclear explosion if one had occurred. Since the radioactivity and heat can be contained within a few hundred feet or less of the underground site of a nuclear explosion, these can only be used to identify it when it has been localized within a very small region. Various geophysical methods have been investigated and it was found that most of them worked rather poorly. No radioactivity will escape. The anomalies in the acceleration of gravity or in magnetic or electromagnetic properties would be negligible in comparison with the naturally occurring variation, making these methods useless for further localization. Seismic profiling seemed to be perhaps more hopeful but could not be carried out in any event over the very large area within which the seismic stations would localize the event. Furthermore, certain conditions such as a high-velocity caprock would make it not terribly useful.

Suppose then that one were inspecting onsite an event which had actually been a nuclear explosion. What would be the probability that one would be able to

localize it sufficiently so that drilling would allow it to be identified as such? The Conference of Experts was unable to answer that question, and that is not surprising. Though further studies has taken place since then the question can still not be answered today. If one reads what the Conference of Experts had to say about this question in its conclusions on the capability of the system one finds that the statement that there would always be some probability of onsite inspection establishing a violation if one had occurred. This is equivalent to saying that the probability is not zero. However—and this is also implied in the same paragraph by the statement that it might be very difficult for the system to catch a violator—the probability might be made very small by careful precautions taken by the violator. Certainly the probability of success of onsite inspection at the site of an actual concealed explosion would not be one if the violator were to take any precautions at all. Whether "very small" means a half, a tenth, a hundredth, or a thousandth, I do not think that anyone could say at the time of the Conference of Experts, nor can they say now. I should reemphasize that the probability of which I have just been speaking is that of success in onsite inspection only. It does not include any degradation produced by inspecting only a fraction of unidentified events, nor does it include the possibility of decoupling so that a nuclear explosion would not be detected at all, and thus could not be inspected.

Perhaps I may digress a moment to say something about how the experts' system came to have 180 stations. If one reads the report of the Conference of Experts, it will be evident that the discussions of the capability of the various methods are all written in terms of 1 kiloton explosions for the methods that were first considered. That is the acoustic method is evaluated in terms of what it will do for 1 kiloton explosions, the radioactive debris method is written as regards the ability of a station to detect 1 kiloton and so forth. When the seismic methods of detection were discussed it became clear that the identification of earthquakes would be possible only with a very large number of stations, if one were to consider earthquakes whose seismic signals were equivalent in amplitude to those of an underground nuclear explosion underground, with Rainier coupling, of 1 kiloton. One cannot identify explosions as such from their seismic signals without the additional action of onsite inspection. One can, however, hope to eliminate a large fraction of the events by identifying them as earthquakes. A natural number to take is 90 percent. This does not mean the system has 90 percent capability of identifying explosions of a particular yield, but that it can identify 90 percent of the earthquakes of equivalent size. To do this for 90 percent of the earthquakes of 1 kiloton equivalent or more required, it turned out, something like 650 stations, worldwide. However, 650 stations were something that the Eastern delegation would not buy. The Western delegation then made the proposal that the number of stations could be adjusted depending upon what capability one wanted to give the system. If one wanted to be able to identify 90 percent of earthquakes of 5 kilotons equivalent with Rainier coupling, the spacing of stations could be increased. This corresponds to the 180-station network finally recommended by the Geneva Conference of Experts. The Western delegation felt that there was a very large number of possible systems of whose capabilities it was prepared to estimate. However, having finally agreed to discuss the capabilities of a 180-station system, the Soviets refused to consider any other. The report of the Conference of Experts is therefore cast in terms of that 180-station network.

Roughly, one could describe the experts' system, in terms of the capability it was believed to have at the time of the experts' conference as a 5-kiloton system so far as underground explosions were concerned. Between 5 kilotons and 1 kiloton its capability fell off very rapidly, being very near zero at 1 kiloton, at which only a fraction of earthquakes were even detected. The report of the Conference of Experts makes it plain that there is a threshold below which the system has very little capability. Subsequent developments have indicated that some capabilities of the system recommended by the Conference of Experts is very much poorer than was believed at that time. If one expresses the capability of the system in terms of identifying earthquakes of a given magnitude, then the capability of the system is moderately worse than believed at that time because of the degradation of the first-motion criterion. If one thinks of the capability of the system in terms of its ability to detect and identify underground explosions and explosions in space, then later data show its capability is very much worse than was thought at that time, because of the possibility of great decreases in coupling which were not then appreciated for underground shots, and the possibility of shielding of explosions in space.

If I may say a final word about the Conference of Experts and its results I would like to note that for most or all of the difficulties which have since come up with regard to the capability of the detection-identification system one can go back to the report of the Conference and point to a passage in which at least some recognition was taken of them. One can say, looking back, that perhaps not enough attention was paid to them, particularly in such items as the possibility of very large decoupling. No specific method of decoupling had at that time been demonstrated. Perhaps it could thus not have been expected to get very much attention. Since a specific method has been demonstrated I think now one should consider not only that method but all the other possible methods for decoupling. The Conference of Experts report can logically be criticized for not taking adequate account of the self-evident fact that a violator of a treaty or a moratorium on nuclear weapons tests must be expected to take full advantage of whatever methods he can find to reduce the probability of detection. It is therefore rather misleading to consider only the probability of detection of an unconcealed shot. We must not ignore the very difficult problem of detection and identification of nuclear explosions which are intended to go unidentified, and for which every possible precaution would be taken to keep them undetected or unidentified.

Representative HOLIFIELD. The next witness will be Dr. Wolfgang H. Panofsky, who was chairman of the Technical Working Group I and his testimony will be related to high altitude test problems. Dr. Panofsky, we are very happy to have you before us.

We are all aware of your excellent scientific background and your contribution. We know that you are the professor of the high-energy physics department at Stanford.

We are glad to welcome you here this morning. We are running a little late, so if you do not finish, we will resume at 2 o'clock with your testimony.

STATEMENT OF DR. WOLFGANG H. PANOFSKY,[1] HIGH-ENERGY PHYSICS LABORATORY, STANFORD UNIVERSITY, STANFORD, CALIF.

Dr. PANOFSKY. Thank you very much. I want to report on the work of Technical Working Group I.[2] The experts considered the detection and identification of nuclear explosions only up to 30–50 km. This upper limit is not related to the maximum distance beyond which nuclear tests are of no concern nor is there any basis for this upper limit from the point of view either of a practical limit on carrying out nuclear tests or of prevention of fallout. We do not have good enough theoretical or experimental knowledge to determine reliably

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Consultant to: 1945 to date, USAEC, Division of Military Application; 1955–57, USAF Scientific Advisory Board; 1951 to date, University of California radiation laboratory, Berkeley; 1954–58, Physics Panel, National Science Foundation; 1958–59, President's Science Advisory Committee; 1960–[63], member, President's Science Advisory Committee; 1959, chairman, ad hoc panel on high-altitude detection; 1960, chairman, ad hoc panel on seismic research and development; 1959, passim, w.a.e. Foreign Service officer (consultant), Department of State: Chairman, U.S. delegation, Technical Working Group on High Altitude Detection, Geneva, June–July; vice chairman, U.S. delegation, Technical Working Group 2, Geneva, November–December; 1960, member ARPA ad hoc group on detection of nuclear explosions; 1958 to date, member, High Energy Commission of International Union Pure and Applied Physics; member Advisory Committee for Conferences held under IUPAP auspices in high-energy physics and instrumentation; 1959, July–December, research visitor, CERN, Geneva.
Professional affiliations: Fellow and member of council, American Physical Society; member National Academy of Sciences; member societies of Sigma XI and Phi Beta Kappa.

[2] The report of Technical Working Group I will be found in app. 3, p. 589.

what a safe upper limit on nuclear detonations would be to prevent any fragments from returning to earth. This question is a very complicated one, and the answer depends in great detail on the state of electrification of the fission fragments and of the magnetic fields in space at large distances from the earth which would tend to trap such fragments. One can surmise that detonations carried out beyond the distance to the moon (about 100,000 miles) would produce almost no debris which would return to the earth.

In discussing explosions in space and their detection, we are dealing with a field where there is a great deal less experience as compared to explosions in the atmosphere and underground. The Conference of Experts in their final report (app. 1, p. 479) specifically discussed the possibility of explosions at much higher altitudes, but stated that they had not considered the matter in detail. Actually, in the work of that Conference there is considerable material bearing on this problem.

I have little doubt that explosions at very large distances from the earth can be carried out, and that they can be used effectively for development of weapons.

Senator GORE. What do you mean by that?

Dr. PANOFSKY. For purposes of this discussion we are talking about distances as far as 200 million miles. That means beyond the distance from the earth to the sun.

Senator GORE. 200 million? You would not talk about just a million miles?

Dr. PANOFSKY. We are considering here the entire range where one might be concerned about another country's developing weapons. I will later discuss briefly what that implies. In order to make sure that we will not start all over again discussing the question, "Why did we only go to a million when you can go to 10 million?" I will say that we have been trying to be as comprehensive as is practical under the circumstances.

There are many kinds of tests that could be carried out in space as well as in the atmosphere, depending on the objectives of the tests. Nuclear tests in space would be carried out by having a test vehicle carry the device to the distance in question, then spread out test pods which would travel about 10 miles from the device, and telemeter signals from the explosion back to earth. The practicality of this procedure is well established in principle, but in a general way difficulties increase as greater and greater distances are considered—primarily because the weight of the pods increases because of telemetry requirements, and also because for a given rocket less weight can be carried as one goes to greater distances.

Nuclear tests in space would limit the extent to which specific kinds of test observations could be used. Nevertheless, in principle the conditions would be entirely adequate for weapons development; in addition, space tests would be useful in cases where the absence of air is an advantage—for example, in connection with development of nuclear weapons as warheads for ballistic missile defense vehicles.

Our only experience directly related to high altitude tests are the shots in the Hardtack series, of which Teak and Orange were the highest. A certain fraction of the experience gained there is of value in discussing tests at high altitudes, but in general the problems would be quite different. A similar remark applies to detection. All the methods of detection which I shall discuss later are based on either

theoretical considerations or a synthesis of various experimental measurements. We have, however, flown radiation detectors in satellites and have sent communication signals over large distances. We are therefore in some phases of the detection problem in a situation similar to that in which we are with the problem of conducting tests in outer space: in both areas we have experience which makes us believe that such problems can be solved, but the actual situation, testing or inspection, has not been tested.

The purpose of Technical Working Group I was to consider explosions at distances greater than 30–50 km., without specifying initially an upper limit on distance. From the point of view of practicality, we believe that such an upper limit should be considered to be several hundred millions of kilometers. This is beyond the distance from the earth to the sun, and present-day rockets would take about a year to carry the equipment for a nuclear test to such a distance. Hence, a test program would be very lengthy because one would have to wait a year after the test vehicle was committed before any test data returned.

In considering the problem of controlling outer space testing, we are faced with basic decisions which must be faced in almost all arms control problems. Testing in outer space represents a new possibility of carrying out a violation, although at considerable expense to the violator. The question of further concealment arises, too, in connection with tests in outer space. The rocket carrying the device could also contain shields which could unfold around the device and reduce the detectable signal. Again, this increases the complexity of carrying out such tests. At the same time, the control system can also be expanded, leading to additional expense and complexity. From the purely technical point of view, it appears likely that, given arbitrarily high incentives on the part of the violator, it will always be possible to devise an essentially undetectable means of carrying out the violation. I believe, however, that before that point is reached, there is the question of whether there really is sufficient incentive either for the violator to carry out tests under these extreme conditions or for the detecting system to be expanded to the maximum possible degree.

The problem of controlling explosions in space is substantially different in respect to providing accurate identification of a nuclear explosion from that of controlling tests in the atmosphere or underground. It is considered that the detection of radioactive debris constitutes identification of an atmospheric test. For detection of underground tests, it has been agreed by the participants in the Conference on the Discontinuance of Nuclear Weapons Tests that seismic signals can be used to sort out a group of suspicious events, but that identification would have to depend on onsite inspection. Since explosions in outer space do not permit the collection of radioactive debris, and also preclude onsite inspection, there is the difficult problem of how reliable the detection of outer space explosions must be to constitute a basis for identification. In the discussions of Technical Working Group I, such problems were not discussed to the same degree of detail as the atmospheric and underground cases had been.

The terms of reference adopted by the Conference were as given in attachment (1) (p. 61) to statement. Note that the phrase, "on the basis of the discussions and conclusions of the Geneva Confer-

ence of Experts," was used. These words were put in at the request of the Soviet delegation. This point is a very significant one in all the technical discussions. After the report of the 1958 Conference of Experts was filed with the governments of the three countries, all of them (including the United States) formally accepted the report. As a result, the U.S.S.R. delegations like to refer to the report as a formal agreement on technical facts rather than as a technical working paper. In so doing, they propose that the experts' report is an unchangeable point of departure for the political negotiators. On the other hand, the United Kingdom and United States delegations have assumed that the technical facts should be brought up to date to reflect changes in our knowledge. You will see that a great deal of our difficulty in these technical negotiations stems from this divergence of view of the role of the experts' report. Specifically, the Russians are generally agreeable to considering changes in the technical situation if they appear to improve the capability of the detection system beyond that specified by the experts. At the same time, any changes which appear to deteriorate the performance are always controversial when introduced into the technical deliberation

Senator GORE. I did not understand that remark, Doctor.

Dr. PANOFSKY. The Russian delegation always takes the attitude that the Conference of Experts' report is a legal rather than a technical document. Now it is stated in the Conference of Experts' report that improvements are possible.

Senator GORE. I understood that.

Mr. PANOFSKY. Therefore, in the subsequent technical negotiations, the one I am reporting here now and the one which is the subject of my next presentation, agreement is always reached when we are proposing new technical facts which appear to improve the system.

Senator GORE. Improve detectability?

Dr. PANOFSKY. Detection capability.

Senator GORE. Do you think they would agree to that?

Dr. PANOFSKY. They will agree to that. But as soon as we introduce technical facts which make the apparent capability for detection and identification of the system less, then they will in general disagree, or at least very thoroughly challenge the technical validity of our statements.

Representative HOSMER. Dr. Panofsky, you say at the same time any changes which appear to deteriorate. Do you mean any newly acquired knowledge?

Dr. PANOFSKY. Any newly acquired knowledge which we would be introducing which would make the system appear less powerful as a means of detecting or identifying nuclear tests.

Representative HOSMER. And this newly acquired knowledge is debated or not accepted.

Dr. PANOFSKY. Either debated, not accepted, or called premature, or whatever the various criticisms might be.

I should like to mention another circumstance of interest in understanding the problems encountered in the technical parts of these negotiations. We all realize that the Russians have very little interest in the technical reliability of control procedures because clandestine testing is essentially impossible in any of the Western countries. They do, however, have a very substantial interest in maintaining the secrecy of their country, and therefore in reducing the degree of access

demanded by the control system. Hence, it is always in their interest to minimize the extent of the control system; in order to do this, the tendency is to view the performance of the control system in the best possible light. Any critical evaluation of the performance of the system, therefore, always rests upon the shoulders of the western delegations. In turn, this makes the performance of the negotiations very arduous for the Western technical delegates since in many cases the facts which have to form the basis for a critical assessment have not been developed prior to negotiations.

Representative HOSMER. Can you give some examples of that?

Dr. PANOFSKY. I think I probably can. This example which I quote is mainly in reference to my second presentation and to the testimony given by Dr. Brown. In discussing in Technical Working Group II on underground detection the question of whether our instruments were suitable or not to the problem, the following question arose: The Russian delegates challenged that the instruments which we had been using during the Hardtack series did not conform to the specifications for such instruments laid down by the Conference of Experts, and, therefore, that our claim for the performance which we described for the system was not valid. It turned out that the criticism of these instruments was not a correct one. It was correct that our instruments did not correspond in detail to the specifications. But it was also correct that their performance was in fact better than the specifications. However, when the Russians brought up this point, we did not know that they were in fact better, but we had to carry out the technical analysis to show that they were better, and we very fortunately were able to do so during the negotiations. We even carried the matter further. We were able to have the two insturments which were being compared produced and set up in the United States.

We were also fortunate enough to have an earthquake occur which made it possible during the time of the negotiations to make a direct comparison on the point in question. This, I think, is probably as extreme an example as you might gather about work on this nature being generated in the course of negotiations rather than in preparation.

Representative HOSMER. I think it is a very excellent example of the quality of the work you and your group have done, Dr. Panofsky.

Senator GORE. Congressman Hosmer, if you would yield, I think the whole Conference serves as an example of the paragraph which Dr. Panofsky has just read. I have watched you in the technical Conference over there, Doctor, with great admiration. Here in this paragraph you have become an English scholar in my opinion. That is the best summary of the technical difficulties of the entire Conference that I have read anywhere.

Representative WESTLAND. Doctor, are you saying that if the Soviet agreed to an inspection system that they would specify the type of equipment that could be used, and that if improvements on that equipment were made here in this country, that they could not be included as a part of that detection system?

Dr. PANOFSKY. At the present status of the treaty draft the situation is as follows: Changes in the system can be made by agreement between all the partners, that is, between all the signatories to the treaty. This means that if changes are desired, then effectively any

one party has a veto right which would govern the introduction of this change. It is for this reason that it is possible to make improvements, but it is also for this reason that any changes which might turn out to be unacceptable to one of the parties concerned would not necessarily be introduced. It is for this reason that in these negotiations we believe that the means of surveillance must be specified to a sufficient degree of detail such that they can be implemented without facing the question of there being such a major change in the system that the problem of unanimous consent would arise.

Representative HOLIFIELD. In other words, what you are really saying is that if we are capable of developing increased detection techniques, you would want them to be put into the stations as quickly as they can be built without the right of a veto of the nation in which they will be installed?

Dr. PANOFSKY. Yes. In general the question is this: There are several kinds of improvements one would consider. Some kind of improvements would deal just with such things as different ways of handling the data or different ways of analyzing the records, or maybe even adding additional instruments to posts already in existence. Those matters may not be controversial. But it is clear that any improvements which would change the degree of access required to the countries involved might very well be controversial. Hence, at least the technical methods which are being considered here have to be specified completely to the extent to which they define the degree of access which would be required in installing and maintaining them, and taking the records.

Representative PRICE. You are also saying that the Russians have a tremendous advantage in this area. They are not as concerned as we are with the development of an adequate and foolproof detection system.

Dr. PANOFSKY. Yes. I am saying that these negotiations are not symmetrical because of the fact that the whole question of adequate control is a concept which we have introduced into the negotiations, and it is our responsibility to analyze what it means both technically and politically. It is for this reason, as you point out, that there is an advantage in these negotiations on the part of the Russian delegation in that they need not be as critical as to the detailed operations of the system.

Representative PRICE. The weaker the system, the better they may like it.

Dr. PANOFSKY. My personal opinion is that they are not as much concerned with the weakness or strength of the system for detection as they are with the degree of access which the system implies. I think this is really the most relevant point.

Representative HOLIFIELD. In their negotiations to date, at least, they have applied the principle of veto of specific on-site inspections, have they not? They have required harmonious agreement?

Senator GORE. That is getting the doctor into something that is out of the scientific field.

Dr. PANOFSKY. I think it is outside my scientific field, but I think it is not correct at the present status.

Representative HOLIFIELD. I will withdraw my question.

Dr. PANOFSKY. Thank you.

After these general remarks, I should like to discuss the work of the Technical Working Group. After considerable discussion, the group adopted the agenda given in attachment 1 (p. 61). As a result of the terms of reference and these agenda, we were in the fortunate position of being able to discuss the technical facts first, then to try to assess the technical performance of possible control systems based on these facts, and only after this to proceed to specific recommendations to the Conference. Note that our group was not to make direct recommendations for the adoption of certain devices into the control system; we were only to recommend such devices for consideration by the political Conference. This meant that, despite the very substantial technical agreement reached by the Technical Working Group, the question was left entirely open to further negotiation of the degree to which either side is willing to expand the experts' control system to include the capability for high-altitude detection.

I would now like to discuss the technical situation in approximate terms. When nuclear explosions take place in outer space, radiations are emitted which can travel very great distances in the vacuum of outer space. Because of the earth's atmosphere, however, only a small fraction of these radiations reaches the ground. As a result, the control of explosions in outer space demands two different types of detection systems, one based on satellites and one located on the ground. The ground-based types are more attractive from the point of view of cost and presumably also of reliability, and because they can be added to the control posts proposed in connection with atmospheric and underground detection. As we shall see, however, their performance is sufficiently limited that satellite-based detectors must also be considered. Nuclear explosions detonated in space emit a large variety of radiations, listed in table I. Of these, only visible light reaches down to the ground; all the others would be absorbed in the upper layer of the atmosphere.

TABLE 1.—*Signals from nuclear weapons in vacuo*

Type:	*Emission time (in microseconds)*
X-rays	0.1 to 1.
Prompt gamma rays	0.03.
Delayed gamma rays	1 second.
Neutrons	Minutes.
Visible light	0.1 to 1.

The types of radiation listed in table 1 are described as follows:

(*a*) X-rays are emitted due to the very high temperature of the nuclear device immediately after detonation.

(*b*) Prompt gamma rays are emitted in a very short time after detonation.

(*c*) Delayed gamma rays are those which continue to be emitted by the radioactive fission fragments for a substantial time after detonation.

(*d*) Neutrons, like gamma rays, may be considered in two groups, prompt and delayed. In the case of delayed neutrons, however, the effective time of emission is on the order of minutes. Unlike other radiations, neutrons travel not at the speed of light but at a variety of lower speeds to arrive at a detector over a substantial time interval, depending on distance from the source.

(*e*) Visible light is due to the incandescence of the nuclear device and is that small part of the thermal radiation emitted in the spectrum visible to the eye.

In addition to the radiations from a nuclear explosion described above, Technical Working Group I considered other secondary effects as possible tools for detection and identification. These were presented by Dr. Panofsky and are described as follows:

(*a*) Visible light produced in the upper atmosphere when the X-rays from the detonation cause it to fluoresce.

(*b*) The change in the properties of the ionosphere due to absorption of energy from a nuclear explosion in space. This change affects the absorption and reflection of radio waves and is detectable, by—

1. The so-called ionospheric radar method.
2. The change in the strength of radio signals received on earth from outer space (cosmic noise receiver).

(*c*) The Argus effect. This name applies to the trapping of electrons in regions of the earth's magnetic field, the electrons having been emitted in the radioactive decay of fission fragments from a nuclear detonation at an altitude of a few hundred miles.

Representative HOSMER. Are these the same type of radiations that come from the sun?

Dr. PANOFSKY. Yes, to some extent. To some extent, no.

Representative HOSMER. What I was getting at is, would they be distinguishable from a natural background?

Dr. PANOFSKY. I will discuss this later on, but I can show in the first slide what I mean. There are several types of radiation considered here. The one giving the largest range is the so-called thermal X-radiation which is emitted by a bomb because its case is at a very high temperature.

To answer your question, approximately 1 percent of the sun's energy is in the form of radiation of a similar nature. However, the amount is such that the ranges which we have calculated take this matter into account.

We have the following types of radiation. We have X-rays which are emitted due to the high temperature of the bomb, so-called thermal X-rays. We have prompt gamma radiation which is emitted in a very short time, about one-hundredth of a millionth of a second during the time of detonation. We then have what we call delayed gamma rays which come from the fission fragments which travel away at high speed from the explosion. Then we have neutrons which are emitted at the time of the explosion but, because they travel at slower speeds than the speed of light—with which all the other three radiations are traveling—are spread out in time by the time they would arrive at a detector. Finally, a small fraction of the energy of the bomb comes off as visible light which also is emitted in a relatively short time. I have given here formulas, which I will not discuss, which tell how many of these various kinds of radiations are emitted by a bomb of a certain yield. Now, I will discuss later the question of background which is essentially the problem raised here, because whenever you have a signal of a certain strength, then the range at which you can detect such a signal would depend on the competition between the natural phenomena which give similar radiations, and the radiations which are given by the thing which you are trying to detect.

In order to design systems of satellites which would be sensitive to these radiations, one must consider the strength of these signals as well as the strength of comparable naturally occurring signals ("background"). Our knowledge of such background radiation is extremely scanty, although it is increasing very rapidly as a result of research carried out by means of satellites and space probes. Since the detectable ranges depend on the competition of the signals from these two sources, any assessment which can be made will be fairly inaccurate.

The existence of these various signals, with the exception of heat radiation in the form of soft X-rays (table I) was discussed by the experts. Our delegation to the Technical Working Group I introduced

the thermal X-ray detection method which appears to have the largest range of detection; we also elaborated in considerable detail on the detection of other signals. As a result of our work previous to the technical working group and of deliberations during the meetings of the group, we were able to agree with the other delegates on devices to be placed in satellites to detect thermal X-rays, prompt gamma rays, delayed gamma rays, and neutrons. The reason for wanting to detect this multiplicity of radiations has to do with a remark made earlier: one wants, in space surveillance, to receive as many signals from the event as possible in order to provide reasonably convincing identification of a nuclear test. There was relatively little disagreement on the facts relating to the performance of these various methods. When the method of detection of thermal X-rays was introduced by the U.S. delegation, there was at first considerable reluctance on the part of the U.S.S.R. delegates to accept it because it was a method not considered by the experts; as soon as they realized it actually constituted a considerable increase in the potential range of control, and therefore improved the control system, all objections disappeared.

After some controversy, the possibility of using shields deployed around the device was also admitted by all parties, and a qualitative description of their effect on the total weight which would have to be carried into space was agreed on.

Representative HOSMER. Dr. Panofsky, may I ask this question? What would be the sense of going to shielding because if an outer space explosion were detected, who could say who caused it?

Dr. PANOFSKY. This is the significant point here. There is no way unless information was also available on vehicle launchings. Of course, each party would know what launchings they had undertaken. But there is no means in space surveillance to connect a given violation with a particular party.

Representative HOSMER. In other words, the actual violator could just blame it on somebody else and there is no way to prove he's lying?

Dr. PANOFSKY. However, there is the point to consider here that this treaty provides no specific sanctions of any kind, as the matter stands now if a violation occurs, beyond the voiding of the treaty. I would only like to mention the fact in connection with your question.

If shields are used, the maximum detectable range can probably be decreased by an order of magnitude, a factor of 10, even for weapons as large as one-half megaton; smaller weapons could be shielded quite effectively for shorter ranges. Added to the complexity of conducting a nuclear explosion in outer space, the observation of the results of such tests by instruments in test pods, and the means of transmitting the information back, the deployment of such shields would of course be a very complicated and expensive undertaking, requiring in itself a great deal of development.

Representative HOLIFIELD. Before you start this paragraph, Doctor, it is 12:30 now, and some of the members have noon engagements. I believe we will ask you to stop at this time, and we will start right at this point at our 2 p.m. meeting.

Dr. PANOFSKY. Very good.

Representative HOLIFIELD. Thank you very much.

The committee is adjourned until 2 p.m. this afternoon.

(Thereupon at 12:30 p.m., a recess was taken until 2 p.m., the same day.)

55625—60—pt. 1——4

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order.

This afternoon we will continue with Dr. Panofsky's testimony on Technical Working Group No. I, which was the high-altitude detection and Technical Working Group II, which has new data to explain to us.

You may go ahead, Dr. Panofsky, on page 6 at the bottom paragraph.

STATEMENT OF DR. WOLFGANG H. PANOFSKY,[1] HIGH ENERGY PHYSICS LABORATORY, STANFORD UNIVERSITY, STANFORD, CALIF.—Resumed

Dr. PANOFSKY. Thank you very much.

I am continuing the testimony on the work of Technical Working Group No. I on high-altitude detection.

There was considerable discussion of the specific orbits to be chosen for the satellites and of the number of satellites required. This arose from several facts: The first is that as a result of discoveries made by the American satellites and supplemented by Russian data, it is now known that there exist several layers of charged particles at altitudes above the earth of between about 1,000 and 20,000 miles—the Van Allen layer.

If detectors carried in satellites move through the Van Allen layer, then the sensitivity of detection would be very much depressed, by a factor of about 10, by the effects of the radiation in the layer. Hence, the satellites should be deployed either below or above the Van Allen layer.

This creates further questions: If the satellites are deployed below the Van Allen layer, then no one of them can see very far around the earth, and a very large number of them would be required to blanket the earth so that no gap in the coverage would exist. We note here that in considering the system we must assume that the orbits of such satellites would be known to other countries involved. If the satellites are to be deployed below the Van Allen layer, there must either be many satellites or the system looking up from the ground must have considerable capability.

If, on the other hand, the satellites are placed at higher altitudes, greater than 20,000 miles, then a smaller number could survey the entire earth—about six—and their view would not be limited by the horizon. Both satellite systems, either the low or the high one, would have the same capability for looking out into space in addition to their capability for surveying the earth.

The high-orbit satellites are technically more difficult to achieve, first because considerably larger vehicles are required to carry the same detectors into the higher orbits, and, second, because such high orbits are very difficult to establish without getting into trouble with the sun's and moon's attraction for the satellites.

In fact, to the best of our knowledge, neither the Russians nor the United States have as yet placed a satellite in an orbit corresponding to the performance required by the Technical Working Group for the high-orbit satellites.

[1] See biographical sketch, p. 35.

I would like to show the two slides showing where the Technical Working Group considered these satellites should be placed.

Figures 1 and 2 indicate the disposition of these two types of orbits, as discussed during the Technical Working Group. After considerable discussion, the group decided that the high-orbit system should be recommended, as the first choice; but the possibility was left open that for economic or technical reasons the lower altitude system might be established first.

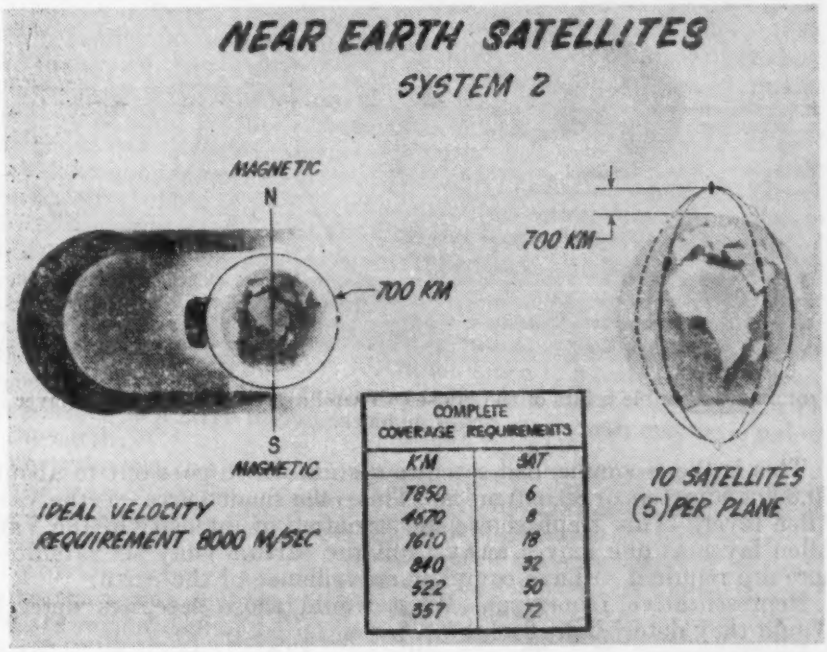

FIGURE 1.—The orbit of the "near-earth satellites" about the earth. The black area is the Van Allen layer described in the text. The table inserted shows the numbers of satellites required to obtain complete coverage down to a given specified altitude.

As you recall, I discussed two kinds of systems of satellites, one to go very close to the earth similar to the ones we and the Russians have launched. Here is the earth. These black spots are the region our detection satellites must stay out of in order not to be blocked by the Van Allen radiation. However, the problem is that, depending on the altitude, in order to be able to survey the earth looking down, if you are low then the horizon will limit your view.

Therefore, the number of satellites which are required to survey the entire earth—and we must always assume that a violator knows where these satellites are—keeps going up. If you wish to look down from the satellite to 357 kilometers, you will need 72 satellites; to 522 kilometers, 50; to 840 kilometers, 32; and so forth. So as you go up in altitude you need fewer satellites.

In the near-earth orbit system we have the problem that you use a large number of satellites or you will have blind spots near the earth.

The next slide, please.

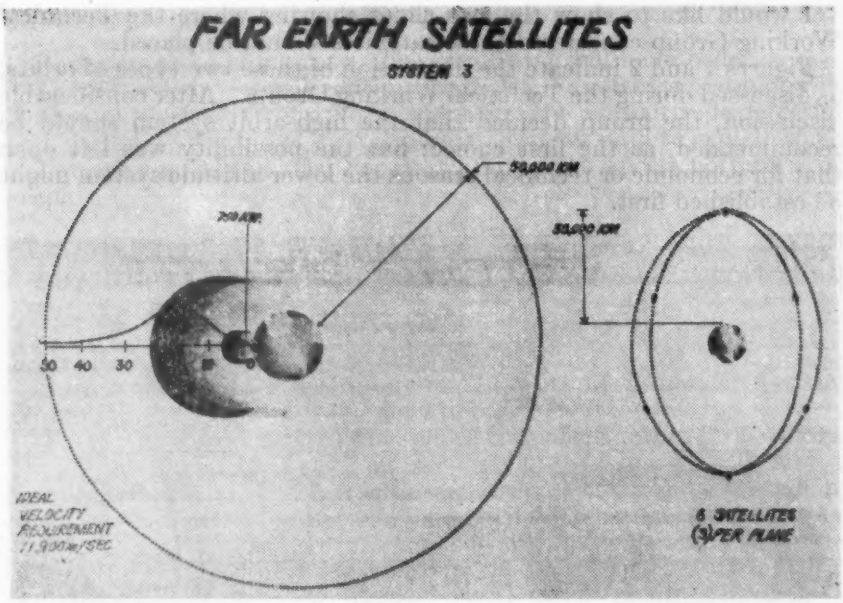

FIGURE 2.—Possible orbits of the "far-earth satellites." The Van Allen layer is shown in black.

This is the recommended satellite system which goes out to about 50,000 kilometers or 30,000 miles. This—the shaded area—is the Van Allen layer. This graph shows the variation of intensity of the Van Allen layer as one moves away from the earth. Only six satellites here are required to have complete surveillance of the earth.

Representative HOLIFIELD. What would those satellites detect? Would they detect light flashes?

Dr. PANOFSKY. No. These satellites would have the equipment for all the types of radiation other than the light. The light radiation detection which I discuss later will be done from the ground. These satellites will carry equipment for neutrons and gamma rays and the so-called thermal X-ray detection which were the top three items which I had in my first table, which give larger detection ranges in general than the light flashes do.

A problem discussed during the Technical Working Group was the possibility of a violator's carrying out tests behind the moon. For this reason, and also in order to make the use of shields more difficult, the group discussed the possibility of solar satellites.

The Russians attempted to minimize the possibility of testing behind the moon, and tried to maximize the difficulties of achieving communications from satellites orbiting the sun back to the earth. Actually, the technical difficulty in deploying solar satellites is in some respects less than that of establishing the high-orbit earthbound system.

The Working Group agreed to include the establishing of solar satellites in its recommendations to the Conference:

In the event that it is deemed necessary * * * a system of four satellites should be placed in appropriate solar orbits * * * when the state of technology permits (app. 3, p. 589.)

It appears in retrospect that the need for the solar satellites is probably smaller, since even if a test was carried out behind the moon the fission fragments from the explosion would drift beyond the limb of the moon in less than a second, and the delay gamma rays from these fragments would remain detectable from earthbound satellites. This means that the only major incentive for solar satellites would be to force the use of more complex shields for concealment.

You have heard of the Argus effect and the successful American tests to demonstrate it. In these tests, three weapons of about 1- to 2-kiloton yield were exploded at an altitude of about 300 miles. The fission fragments from the explosions produced electrons by radioactive decay, and these electrons were trapped in the earth's magnetic field where they remained for a very long time, of the order of 1 week, depending on the details of the altitude and energy of the charged decay particles.

Such trapped particles can be detected by radiation counters carried in a satellite orbiting in this belt. This detection method is a very powerful one since a single satellite can patrol the whole region in which the charged particles are trapped and can sample the layer many times as the satellite circulates.

For this reason, the Working Group recommended that a single satellite for this purpose should be included in the control system. Unfortunately, this method is restricted since it covers only the specific region in which the trapping occurs, and since this trapping is not possible in a large region over the north and south magnetic poles of the earth.

Nevertheless, this method makes it possible to deny an otherwise very attractive region of space to a potential violator with only a single satellite and relatively simple instrumentation.

As mentioned earlier, the satellite-borne detectors will in general not provide coverage against tests carried out close to the earth but above the region included in the 1958 experts' report. This intermediate region cannot be neglected, in particular since considerably greater weights can be lifted to altitudes of, say, 1,000 miles than can be lifted to hundreds of thousands of miles. For this reason the group also worked on detection systems based on the ground.

Two of the systems involved observation of visible light which could be received on the earth after an explosion occurred at distances up to about 100,000 miles. These light signals would originate either from the explosion itself or from light produced in the upper atmosphere by the X-rays emitted by the device.

It turns out that these two sources of light are independently useful since the direct light from the explosion would be very difficult to shield since the shield itself would become so hot that it would emit light also. On the other hand, the detection of light directly from the explosion is possible only in clear weather; for distant explosions, however, it would be extremely improbable that all control posts which could see the event were hidden by cloud cover.

The light produced by X-rays from the bomb in the upper atmosphere, however, is such that it will penetrate clouds without much loss, and it could be seen by instruments even in cloudy weather.

Other methods have to do with the influence of an explosion on the ionosphere, a layer about the earth which contains free electrical charges and which, therefore, affects the propagation of radio waves.

The ionosphere makes radio communication beyond the horizon possible by reflecting radio waves of sufficiently long frequencies.

The energy from a nuclear explosion at distances of many thousands of miles deposited in the ionosphere is sufficient to modify its properties so that the absorption and reflection of radio waves will change. This phenomenon can be made use of in two ways: (1) by the so-called ionospheric radar method; and (2) by the fact that the strength of radio signals from outer space received on the earth is changed. The second method was discussed earlier by the experts.

Both of these methods have been studied experimentally in connection with the Orange and Teak shots of the Hardtack series, and also in the Argus experiments. These two methods are somewhat comparable in performance, although the ionospheric radar method is probably more effective for weapons of smaller yields.

When discussing these methods, we encountered, unexpectedly, opposition from the Russian delegates to including the ionospheric radar method in even our technical evaluation.

After very extensive discussions, in which the Russians tried to attack the technical validity of the method, it became clear that their opposition derived primarily from concern that the method might detect other phenomena of military interest, such as missiles passing through the ionosphere.

We succeeded in reaching agreement on a joint technical evaluation of the ionospheric radar method, but not on the recommendation for incorporation into the proposals for consideration by the Conference.

I should now like to summarize the technical situation by showing a chart which gives the distances from the earth covered by these various methods—figure 3. You will note that we are discussing here a technically very complex system and the complexity derives directly from our desire to obtain coverage at all altitudes starting from those considered by the experts up to hundreds of millions of miles.

Because of the blind spots of satellite systems near the earth, the problem of achieving coverage near the earth can be resolved only by the use of the large number of methods we have discussed.

FIGURE 3. THE DETECTION RANGES OF THE VARIOUS METHODS DISCUSSED BY TECHNICAL WORKING GROUP I.

Techniques for detecting high-altitude and space explosions

Method (at control posts):	*Approximate theoretical range (miles)*
1. Direct optical	300, 000
2. Fluorescence	500, 000
3. Backscatter radar	3, 000
4. Cosmic noise absorption	1, 000–10, 000
5. Electromagnetic pulse	3, 000
In earth or solar satellites:	
1. Thermal X-rays	200, 000, 000
2. Prompt gamma rays	300, 000
3. Delayed gamma rays	200, 000
4. Prompt neutrons	100, 000
5. Delayed neutrons	10, 000
6. Trapped electrons	30, 000

NOTE 1.—At distances beyond 200,000 miles, only the X-ray technique will record radiation from a nuclear explosion.

NOTE 2.—The X-ray technique can be shielded to degrade the range. Shielding would reduce the above range by a factor of 10 or more for explosions in the megaton yield range and by a factor of 100 or more for explosions of the order of 10 kilotons in yield.

I have here a chart in which I am putting distance in kilometers. This is 1, 10, and on up to 100 million and 1,000 million kilometers distant. I am showing here that the different methods which I have been discussing occur at different altitudes. The experts' system goes up to 30–50 kilometers; that is as far as their system goes.

Our interest goes from 30–50 kilometers as far as testing might be conceivable. Here the thermal X-ray method, if no shields are used, would cover all the way to the distance of the sun. If shields are used, then depending on details and on the size of the weapon, one would lose some number such as a factor of 10 in range for very large yield weapons and a factor of 100 or more for smaller yield weapons.

Here are the shielded and unshielded detection ranges. This—the X-ray method—is a very powerful method. However, the shielding reduces ranges to below 100 million kilometers. The prompt gammas cover a range of several hundred thousand kilometers. The delayed gammas have the same range and have the great advantage of not being shieldable by any method anybody has been able to think of because of the fact that these radiations come from fission fragments which are being thrown out by the weapons and are not part of the original explosion.

Then we have the prompt neutrons, delayed neutrons, and finally the Argus system I discussed. The Argus system uses the trapped electrons, but it has no coverage over the north and south magnetic poles. Then there are the satellite systems. Then there are the systems deployed in the control posts which were already recommended by the experts. Then there is the so-called direct optical method where you detect the light flash from the bomb; this is valid from several times 100,000 kilometers down to about 1,000. Then there is the so-called fluorescence method that goes a little bit higher but is subject to shielding. Then the controversial back-scatter radar item to which the Russians did not agree. Then, finally, there is the so-called cosmic noise absorption which is a method where one observes the intensity of radio signals which come from outer space and see how they change as the result of explosion.

As you see, we have a very large number of methods here—altogether 10. But the reason we considered them to be necessary is that they supplement one another regarding their performance of coverage in altitude and regarding their response to shielding, and also because of the fact that identification is at best a very difficult problem even if you do achieve detection in this situation.

So our report is summarized by considering these 10 different methods, of which five would be carried in a single satellite and one—trapped electrons—would be carried in another satellite which goes around in the trapping layer around the earth.

Despite the disagreement on the question of including the ionospheric radar method and the objection to the large number of methods, I believe in general the outcome of the Technical Working Group was quite satisfactory regarding the degree of agreement achieved. Also, I believe it did establish that detection is technically possible to the largest ranges considered, and that only complex shielding measures could counteract this capability and then only at very great distances.

Many questions concerning detection of explosions at high altitudes were not answered by the Technical Working Group, such as the

problems involved in decisionmaking as a result of signals received, in procedures for using vehicles for launching satellites for this purpose, and in ascertaining that the satellites were operating properly.

In addition, the system considered involves a large expenditure of money and of technical manpower, and will take considerable time to achieve. Hence, the question as to what extent the control system is to be expanded to provide coverage for detecting explosions in space is still open.

This concludes my testimony on this work.

Representative HOLIFIELD. Now, Dr. Panofsky, before we go into the next presentation which you are going to share with Dr. Brown, would you discuss in general what each control post would look like and what it would consist of in the way of machinery or devices?

Dr. PANOFSKY. You mean if it were agreed to incorporate these devices?

Representative HOLIFIELD. Let us start with the type of devices we have now for detection purposes and assume that the 180 stations were set up according to the working papers. What type of instruments would we have in those stations?

Dr. PANOFSKY. In addition to the ones agreed by the experts?

Representative HOLIFIELD. Yes.

Dr. PANOFSKY. The additional instruments——

Representative HOLIFIELD. First give us what was agreed by the experts.

Dr. PANOFSKY. What was agreed by the experts was a series of 10 short-period seismometers and then some long-period seismometers; then the various devices for sampling of the air for detection of radioactive materials; then acoustic and electromagnetic detection devices.

Representative HOLIFIELD. Will you explain what those are?

Dr. PANOFSKY. When a bomb explodes there is emitted a long-wave radio signal and certain types of radio receivers designed for the reception of very long waves would receive such things. They are essentially radio receivers of a very special kind.

Representative HOLIFIELD. Are all those instruments in existence at the present time?

Dr. PANOFSKY. Prototypes of such things are in existence. But a complete sample station has not been built.

Representative HOLIFIELD. You say prototypes. Have the prototypes been tested to the point where you have any considerable data upon which there is agreement as to its interpretation?

Dr. PANOFSKY. Yes. The technical data which were being used by the experts were actual observations made with seismometers and with radio receivers and the corresponding devices.

The specific stations which have been agreed on by the experts, have not as yet been built. Possibly some of the other witnesses here can discuss in more detail what the detailed experience is. I am not familiar with the detailed program of the low frequency electromagnetic and acoustic measurements, but these are all instruments which do not go beyond the experience which we have already had in principle.

Representative HOLIFIELD. Would your proposal, to go into the high altitude distances as you testified, require the development and manufacture of additional instruments which are at this time not in existence?

Dr. PANOFSKY. That is correct. The devices for the high altitude observation are all instruments which are not in existence other than that they would be made up of components which are in existence.

As an example, in order to observe the light flashes, there has to be built and tested a group of detectors which have many facets looking out into space. They would have to be developed and tested and installed.

Representative HOLIFIELD. Can you give us an estimate as to the time period that would be involved in developing these instruments and testing them?

Dr. PANOFSKY. I take it we are still talking about the ground-based system. The general time periods for developing such a thing have been estimated to be of the general order of 2 years; these things, as you know, depend very much on the competition with other activities requiring similar talents.

The order of time in which one could develop these things is approximately 2 years. The ground-post equipment is not a substantial extension of present art.

Representative HOLIFIELD. You believe that they could be built if they were given the proper priority and in such quantities as would be needed for these 180 stations within a period of 2 years.

Dr. PANOFSKY. It would take a very large effort to do so, but I think this is in principle possible; yes, sir. The remark I would like to make, however, is that after that period one would have to have operating experience to evaluate whether the performance is the way one has calculated here.

Just because of the fact that neither in satellites nor on the ground have these instruments ever been built or tested, we are talking entirely about a theoretical system at the present moment.

Representative HOLIFIELD. As far as the altitude detection is concerned, this is entirely theoretical at this time.

Dr. PANOFSKY. That is correct.

Representative HOLIFIELD. But as far as the ground control stations are concerned, they are not theoretical. They are in existence in their component parts at this time.

Dr. PANOFSKY. That is correct.

Representative HOLIFIELD. So on my question to you as to time, your answer applied to the ground test stations and not to testing devices or satellites which might be needed for the far off space type of detection.

Dr. PANOFSKY. That is correct.

Representative HOLIFIELD. Would you hazard a guess as to the time involved there?

Dr. PANOFSKY. We have made studies of this and in this case the time scale depends a great deal on the detailed administrative arrangements which would be made.

As an example, do we base a time scale on our own vehicles or do we assume that the other signatories to the treaty contribute theirs? We have made an analysis and the details I cannot give here. The answer depends also on which of the systems we are using, namely, whether we would be satisfied with a near-earth satellite or the far-earth satellite, and whether we would include the solar satellite systems.

The estimates in very rough numbers are that one talks about 2 to 3 years for the near-earth satellites and 2 additional years for the far-earth or solar satellites. So it is a total amount of something like 5 years that is a reasonable number.

Representative HOLIFIELD. We are going into that in more detail later on in the program, but I thought it would be well to bring that point out at this time. In other words, we are talking in terms of 4 or 5 years when we are talking about the instruments which have not yet been invented or tested. We are talking about 2 years in the use of the instruments which have at least their component parts—they may not be assembled together as we would assemble them—for that type of detection system.

Dr. PANOFSKY. That is correct. I would like to emphasize again that I am talking about technical leadtimes. These can be superimposed on any other leadtimes which might arise in getting the systems into being.

Representative HOLIFIELD. This would be predicated on an aggressive program of development unobstructed by political factors.

Dr. PANOFSKY. That is right.

Representative HOLIFIELD. We have offered to engage in a research and development program with the Soviets. If our experience in negotiating with them is any criterion to go by, we would not anticipate a smooth-running development program. We might anticipate differences of opinion and delays which could be brought about by failure to agree on the type of research and development program and the different stages; is that not true?

Dr. PANOFSKY. The program which we have proposed to do covers the seismic areas and not these under discussion here at the present moment. Our present proposal is that this shall be a coordinated program and not a joint program. This means that we are proposing to go ahead in doing the necessary research and development and promise to coordinate the activities with the other parties, but our own part of the program will not be held up as the result of lack of agreement in cases where differences of opinion exist as to what topics should or should not be included in the program. It is for that reason that we proposed a coordinated rather than a joint program.

Representative HOLIFIELD. I think this is very important because if this is worth doing it is worth doing on a priority basis, and it is worth doing on a basis whereby the tempo of the research and development would not be obstructed by political arguments and postponements and conferences and adjournments and that sort of thing that we became so accustomed to in the conferences on the ending of the war in Korea and more recently on the test cessation conferences and the disarmament conferences.

I am heartened by your saying that a program of research and development would be within our control and the tempo of it would be within our control and could not be obstructed by outside interests.

Dr. PANOFSKY. That is correct. However, I would like to emphasize that we are talking here about a research and development program in seismic methods. This is the only one which is at present a subject of discussion.

Representative HOLIFIELD. That is right. We understand that background radiations can, by chance, produce a signal similar to an

explosion. Can you say whether there are natural events which produce signals similar to explosions which are not simply chance fluctuations of the background radiations?

Dr. PANOFSKY. The situation there is, of course, not as well known as in the corresponding situation on the earth simply because our experience on these background radiations has just begun recently as the result of our advances in space probes and satellites.

Our main concern with background is not that it gives very short-time bursts which will be mistaken for an explosion, but that, as the background radiation increases, the sensitivity of the instruments would decrease.

One of the provisions which is necessary for the satellite detectors is to incorporate information-transmitting devices which tell us what the background is at the same time as the signal. So even if we are in the region of high background, the instrument can tell us that its performance has degenerated. Therefore, that particular instance of time when the satellite goes to such a region of high background radiation, we will know that during this time it could not see as far.

It is for that reason, of course, that a larger number of satellites is suggested here than the minimum. So it is much less likely that all of them will find themselves in the region of high background. For that reason, the capability of the system is larger.

However, I would like to emphasize that our estimates are based on best present knowledge, and since the age of space exploration is just commencing now, it is, of course, possible that there are surprises there.

For example, the discovery of the Van Allen layer which limited the orbits where the satellites can be deployed was essentially one of those surprises.

Representative HOLIFIELD. Can I ask you if there are natural explosions such as the explosion of a meteorite or some other type of explosion which might be confused with a manmade explosion?

Dr. PANOFSKY. I am not acquainted with such.

Representative HOLIFIELD. Now, as I understand it, Doctor, explosions between about 1 million kilometers and 100 million kilometers from a satellite would be detected only by thermal X-rays.

Dr. PANOFSKY. Yes.

Representative HOLIFIELD. Do you consider that the observation of a single short pulse of X-rays as the only evidence of an explosion without any correlating data from other techniques would constitute a reliable determination that a violation had occurred?

Dr. PANOFSKY. Certainly not if it was seen by only one satellite. If it was seen by several satellites I think it would be considered to be such.

As I mentioned in my main presentation, the whole question as to what shall or shall not be a reliable identification is one thing which was not discussed by the conference at all. It is a very significant omission and certainly must be agreed upon if this area is to be included in the system.

Representative HOLIFIELD. In other words, you feel that this area would have to be explored.

Dr. PANOFSKY. Certainly there will have to be some agreement as to what the definition of identification is. This has not been dis-

cussed at all in this working group. As I mentioned in my main statement, the degree of detail with which questions of this kind were considered——

Representative HOLIFIELD. Are you confining this answer to the space problem or the ground control problem?

Dr. PANOFSKY. Space control problem.

Representative HOLIFIELD. May I ask you if there is an agreement on the interpretation of seismological recordings between the East and the West on the ground-station type of machinery which we have now?

Dr. PANOFSKY. Excuse me. I do not think I understood the question.

Representative HOLIFIELD. Is there agreement as to the interpretation of the recordings of these instruments at the present time?

Dr. PANOFSKY. Not a formal agreement; no. This is a matter which I will come to in my next presentation. There was an attempt in the Technical Working Group II to agree not on criteria for identification, but on criteria for eligibility for on-site inspection; the agreement is that the discovery of radioactivity would be identification.

The question as to criteria when a given event may be looked at was the attempt of the second working group and no agreement was reached.

Representative HOLIFIELD. Assuming that I have asked you an unanswerable question, if we get it into the next presentation we will postpone it at the present time, but what I am trying to determine in my questions is this, given a specific recording on a machine such as I present here to you, would there be an argument as to what that would indicate or is there general agreement as to the definition of the interpretation of what that would mean?

Dr. PANOFSKY. There is at present not general agreement. I will discuss that in the next presentation.

Representative HOLIFIELD. Very well. Are there any questions following the first presentation?

Representative HOMER. Yes. I have one following Mr. Holifield's questioning on the development of equipment in these devices.

What was discussed in regard to communicating any evidence of a suspicious manifestation from one of those stations, say, deep in the heart of the United States to headquarters in, say, Geneva? How would that be accomplished?

Dr. PANOFSKY. Are you talking about communication from space to the stations?

Representative HOSMER. I am talking about communications from stations to headquarters.

Dr. PANOFSKY. This has been discussed to some extent, but not as part of this working group here. The working group which I have been reporting on dealt entirely with high altitude problems. There has been some discussion, but not extensive discussion as yet on the communication network which goes with the ground stations.

Representative HOSMER. Do you believe it would be important that the communication system be of no greater unreliability than the detection system?

Dr. PANOFSKY. That is right.

Representative HOLIFIELD. Mr. Price?

Representative PRICE. Dr. Panofsky, how long do you think it would take to install 180 control posts for monitoring with the optical system?

Dr. PANOFSKY. Including the optical system?

Representative PRICE. Yes.

Dr. PANOFSKY. I believe that the optical system part would not in any way define the time scale of installing the system because if it were decided that it is to be included, its development can go in parallel with the installation of the main system.

So it would not constitute the leadtime. To get the system into worldwide establishment, various estimates have been made, but there exists at present no agreement on it. Before there might be a worldwide system one does talk about periods of 4 or 5 years.

Representative PRICE. Would you rely on this system without an operational test using nuclear explosions in space to prove your theoretical predictions on detection range?

Dr. PANOFSKY. We have not thought about that in detail. We would certainly prefer to have a nuclear explosion in space for testing. The situation here is not quite as difficult a one, possibly, as the underground situation, because the radiations which do come from the weapons are very well understood.

Our greatest uncertainties here are the natural sources. The question concerning the signals from the weapon is quite unambiguous.

Representative PRICE. Is your answer that you place some reliance on it?

Dr. PANOFSKY. I would prefer an explosion for testing but I would not consider it an absolutely necessary condition. I would certainly prefer it to have a higher degree of confidence.

Representative PRICE. That is all I have, Mr. Chairman.

Representative HOLIFIELD. You spoke of detecting in 1 second a shot that might be fired behind the moon because of the speed of fission products. Of course, this would apply only if the explosion was not accompanied by shielding, would it not?

Dr. PANOFSKY. No.

Representative HOLIFIELD. In other words, if it were shielded would it slow down the flight of the fission products?

Dr. PANOFSKY. This particular method would not be affected by shielding because the fission fragments are part of the basic fissionable material source of the bomb which flies apart and the shield would also fly apart and hence the fission products could not be contained.

Representative HOLIFIELD. What you are really saying is that there could be no effective shielding of a test under those circumstances.

Dr. PANOFSKY. For that particular detection method. However, this particular detection method, if you will refer to the last table, has a maximum range of about 200,000 to 300,000 kilometers for 1-kiloton explosions. Therefore, for the very largest ranges, for the hundreds of millions of kilometers, this particular method would not be effective simply for reasons of intensity.

Representative HOLIFIELD. Mr. Ramey?

Mr. RAMEY. You indicated, Dr. Panofsky, that the shielding of nuclear explosions in space is complicated. Do you believe the satellite detection system is simple or is it as complicated as the shielding problem?

Dr. PANOFSKY. They are both complicated. We are talking here about a situation where both the testing and the detection are complicated. One asks frequently the question, "What costs more?" or the question which I believe is more relevant, "How much would the cost of having to carry out explosions in space increase relative to doing such explosions under the most favorable conditions?"

Again, this is a very broad question and the answer depends on details, but the estimate which has been made, which is based on only a rough study, is that the cost of carrying out tests if they have to be carried out far in space might be increased by a factor of 5 or 10, and that the time it would take to accomplish test objectives would be stretched out by something like a factor of 2.

But these studies, of course, are very rough and one can get a large variety of answers depending on the assumptions made.

Representative HOLIFIELD. Thank you very much, Doctor.

(Dr. Panofsky's formal statement on Technical Working Group I, follows:)

A PRESENTATION OF THE RESULTS OF TECHNICAL WORKING GROUP I
(HIGH ALTITUDE)

The 1958 Conference of Experts considered in detail the detection and identification of nuclear explosions only up to 30–50 kilometers. This upper limit is not related to a maximum distance beyond which nuclear tests are of no concern, nor is there any basis for this upper limit from the point of view either of a practical limit on carrying out nuclear tests or of prevention of fallout. We do not have good enough theoretical or experimental knowledge to determine reliably what a safe upper limit on nuclear detonations would be to prevent any fragments from returning to earth. This question is a very complicated one, and the answer depends in great detail on the state of electrification of the fission fragments and of the magnetic fields in space at large distances from the earth which would tend to trap such fragments. One can surmise that detonations carried out beyond the distance to the Moon (about 100,000 miles) would produce almost no debris which would return to the Earth.

In discussing explosions in space and their detection, we are dealing with a field where there is a great deal less experience as compared to explosions in the atmosphere and underground. The Conference of Experts in their final report specifically discussed the possibility of explosions at much higher altitudes, but stated that they had not considered the matter in detail. Actually, in the work of that Conference there is considerable material bearing on this problem.

I have little doubt that explosions at very large distances from the earth can be carried out, and that they can be used effectively for development of weapons. There are many kinds of tests that could be carried out in space as well as in the atmosphere, depending on the objectives of the tests. Nuclear tests in space would be carried out by having a test vehicle carry the device to the distance in question, then spread out test pods which would travel about 10 miles from the device, and telemeter signals from the explosion back to earth. The practicality of this procedure is well established in principle, but in a general way difficulties increase as greater and greater distances are considered—primarily because the weight of the pods increases because of telemetry requirements, and also because for a given rocket less weight can be carried as one goes to greater distances.

Nuclear tests in space would limit the extent to which specific kinds of test observations could be used. Nevertheless, in principle the conditions would be entirely adequate for weapons development; in addition, space tests would be useful in cases where the absence of air is an advantage—for example, in connection with development of nuclear weapons as warheads for ballistic missile defense vehicles.

Our only experience directly related to high-altitude tests are the shots in the Hardtack series, of which Teak and Orange were the highest. A certain fraction of the experience gained there is of value in discussing tests at high altitudes, but in general the problems would be quite different. A similar remark applies to detection. All the methods of detection which I shall discuss later are based on either theoretical considerations or a synthesis of various experimental measurements. We have, however, flown radiation detectors in satellites and have sent

communication signals over large distances. We are therefore in some phases of the detection problem in a situation similar to that in which we are with the problem of conducting tests in outer space: in both areas we have experience which makes us believe that such problems can be solved, but the actual situation has not been tested.

The purpose of Technical Working Group I was to consider explosions at distances greater than 30–50 kilometers, without specifying initially an upper limit on distance. From the point of view of practicality, we believe that such an upper limit should be considered to be several hundred millions of kilometers. This is beyond the distance from the earth to the sun, and present-day rockets would take about a year to carry the equipment for a nuclear test to such a distance. Hence, a test program would be very lengthy because one would have to wait a year after the test vehicle was committed before any test data returned.

In considering the problem of controlling outer-space testing, we are faced with basic decisions which must be faced in almost all arms-control problems. Testing in outer space represents a new possibility of carrying out a violation, although at considerable expense to the violator. The question of further concealment arises, too, in connection with tests in outer space. The rocket carrying the device could also contain shields which could unfold around the device and reduce the detectable signal. Again, this increases the complexity of carrying out such tests. At the same time, the control system can also be expanded, leading to additional expense and complexity. From the purely technical point of view, it appears likely that, given arbitrarily high incentives on the part of the violator, it will always be possible to devise an essentialy undetectable means of carrying out the violation. I believe, however, that before that point is reached, there is the question of whether there really is sufficient incentive either for the violator to carry out tests under these extreme conditions or for the detecting system to be expanded to the maximum possible degree.

The problem of controlling explosions in space is substantially different in respect to providing accurate identification of a nuclear explosion from that of controlling tests in the atmosphere or underground. It is considered that the detection of radioactive debris constitutes identification of an atmospheric test. For detection of underground tests, it has been agreed by the participants in the Conference on the Discontinuance of Nuclear Weapons Tests that seismic signals can be used to sort out a group of suspicious events, but that identification would have to depend on onsite inspection. Since explosions in outer space do not permit the collection of radioactive debris, and also preclude onsite inspection, there is the difficult problem of how reliable the detection of outer-space explosions must be to constitute a basis for identification. In the discussions of Technical Working Group I, such problems were not discussed to the same degree of detail as the atmospheric and underground cases had been.

The terms of reference adopted by the Conference were as given in attachment (1) to this statement. Note that the phrase, "on the basis of the discussions and conclusions of the Geneva Conference of Experts," was used. These words were put in at the request of the Soviet delegation. This point is a very significant one in all the technical negotiations. After the report of the 1958 Conference of Experts was filed with the governments of the three countries, all of them (including the United States) formally accepted the report. As a result, the U.S.S.R. delegations like to refer to the report as a formal agreement on technical facts rather than as a technical working paper. In so doing, they propose that the experts' report is an unchangeable point of departure for the political negotiators. On the other hand, the United Kingdom and U.S. delegations have assumed that the technical facts should be brought up to date to reflect changes in our knowledge. You will see that a great deal of our difficulty in these technical negotiations stems from this divergence of view of the role of the experts' report. Specifically, the Russians are generally agreeable to considering changes in the technical situation if they appear to improve the capability of the detection system beyond that specified by the experts. Hence, they insisted on the phrase "on the basis of the * * * Conference of Experts" in the terms of reference. At the same time, any changes which appear to deteriorate the performance are always controversial when introduced into the technical deliberations.

I should like to mention another circumstance of interest in understanding the problems encountered in the technical parts of these negotiations. We all realize that the Russians have very little interest in the technical reliability of control procedures because clandestine testing is essentially impossible in any of the Western countries. They do, however, have a very substantial interest in maintaining the secrecy of their country, and therefore in reducing the degree of

access demanded by the control system. Hence, it is always in their interest to minimize the extent of the control system; in order to do this, the tendency is to view the performance of the control system in the best possible light. Any critical evaluation of the performance of the system, therefore, always rests upon the shoulders of the Western delegations. In turn, this makes the performance of the negotiations very arduous for the Western technical delegates since in many cases the facts which have to form the basis for a critical assessment have not been developed.

After these general remarks, I should like to discuss the work of the Technical Working Group. After considerable discussion, the group adopted the agenda given in attachment (1). As a result of the terms of reference and these agenda, we were in the fortunate position of being able to discuss the technical facts first, then to try to assess the technical performance of possible control systems based on these facts, and only after this to proceed to specific recommendations to the Conference. Note that our Group was not to make direct recommendations for the adoption of certain devices into the control system; we were only to recommend such devices for consideration by the political Conference. This meant that, despite the very substantial technical agreement reached by the Technical Working Group, the question was left entirely open to further negotiation of the degree to which either side is willing to expand the experts' control system to include capability for high-altitude detection.

I would now like to discuss the technical situation in approximate terms. When nuclear explosions take place in outer space, radiations are emitted which can travel very great distances in the vacuum of outer space. Because of the earth's atmosphere, however, only a small fraction of these radiations reaches to the ground. As a result, the control of explosions in outer space demands two different types of detection systems, one based on satellites and one located on the ground. The ground-based types are more attractive from the point of view of cost and presumably also of reliability, and because they can be added to the control posts proposed in connection with atmospheric and underground detection. As we shall see, however, their performance is sufficiently limited that satellite-based detectors must also be considered. Nuclear explosions detonated in space emit a large variety of radiations, listed in table I. Of these, only visible light reaches down to the ground; all the others would be absorbed in the upper layer of the atmosphere.

In order to design systems of satellites which would be sensitive to these radiations, one must consider the strength of these signals as well as the strength of comparable naturally occurring signals ("background"). Our knowledge of such background radiation is extremely scanty, although it is increasing very rapidly as a result of research carried out by means of satellites and space probes. Since the detectable ranges depend on the competition of the signals from these two sources, any assessment which can be made will be fairly inaccurate.

The existence of these various signals, with the exception of heat radiation in the form of soft X-rays (table I), was discussed by the experts. Our delegation to the Technical Working Group I introduced the thermal X-ray detection method which appears to have the largest range of detection; we also elaborated in considerable detail on the detection of the other signals. As a result of our work previous to the Technical Working Group and of deliberations during the meetings of the Group, we were able to agree with the other delegates on devices to be placed in satellites to detect thermal X-rays, prompt γ-rays, delayed γ-rays, and neutrons. The reason for wanting to detect this multiplicity of radiations has to do with a remark made earlier: One wants, in space surveillance, to receive as many signals from the event as possible in order to provide reasonable convincing identification of a nuclear test. There was relatively little disagreement on the facts relating to the performance of these various methods. When the method of detection of thermal X-rays was introduced by the U.S. delegation, there was at first considerable reluctance on the part of the U.S.S.R. delegates to accept it because it was a method not considered by the experts; as soon as they realized it actually constituted a considerable increase in the potential range of control, and therefore improved the control system, all objections disappeared.

After some controversy, the possibility of using shields deployed around the device was also admitted by all parties, and a qualitative description of their effect on the total weight which would have to be carried into space was agreed on.

If shields are used, the maximum detectable range can probably be decreased by an order of magnitude, even for weapons as large as one-half megaton; smaller weapons could be shielded quite effectively for shorter ranges. Added to the complexity of conducting a nuclear explosion in outer space, the observation of the results of such tests by instruments in test pods, and the means of transmit-

ting the information back, the deployment of such shields would of course be a very complicated and expensive undertaking, requiring in itself a great deal of development.

There was considerable discussion of the specific orbits to be chosen for the satellites and of the number of satellites required. This arose from several facts: The first is that as a result of discoveries made by the American satellites and supplemented by Russian data, it is now known that there exist several layers of charged particles at altitudes above the earth of between about 1,000 and 20,000 miles ("Van Allen layer"). If detectors carried in satellites move through the Van Allen layer, then the sensitivity of detection would be very much depressed by the effects of the radiation in the layer. Hence, the satellites should be deployed either below or above the Van Allen layer. This creates further questions: If the satellites are deployed below the Van Allen layer, then no one of them can see very far around the earth, and a very large number of them would be required to blanket the earth so that no gap in the coverage would exist. We note here that in considering the system we must assume that the orbits of such satellites would be known to other countries involved. If the satellites are to be deployed below the Van Allen layer, there must either be many satellites or the system looking up from the ground must have considerable capability. If, on the other hand, the satellites are placed at higher altitudes (greater than 20,000 miles), then a smaller number could survey the entire earth (about six) and their view would not be limited by the horizon. Both satellite systems, either the low or the high one, would have the same capability for looking out into space in addition to their capability for surveying the earth.

The high-orbit satellites are technically more difficult to achieve: First, because considerably larger vehicles are required to carry the same detectors into the higher orbits, and second, because such high orbits are very difficult to establish without getting into trouble with the sun's and moon's attraction for the satellites. In fact, to the best of our knowledge, neither the Russians nor the United States have as yet placed a satellite in an orbit corresponding to the performance required by the Technical Working Group for the high-orbit satellites.

Figures 1 and 2 (pp. 45 and 46) indicate the disposition of these two types of orbits, as discussed during the Technical Working Group. After considerable discussion, the group decided that the high-orbit system should be recommended, as the first choice; but the possibility was left open that for economic or technical reasons the lower-altitude system might be established first.

A problem discussed during the Technical Working Group was the possibility of a violator's carrying out tests behind the moon. For this reason, and also in order to make the use of shields more difficult, the group discussed the possibility of solar satellites. The Russians attempted to minimize the possibility of testing behind the moon, and tried to maximize the difficulties of achieving communications from satellites orbiting the sun and back to the earth. Actually, the technical difficulty in deploying solar satellites is in some respects less than those of establishing the high-orbit earthbound system. The Working Group agreed to include the establishing of solar satellites in its recommendations to the Conference: "In the event that it is deemed necessary * * * a system of four satellites should be placed in appropriate solar orbits * * * when the state of technology permits," (app. 3, p. 589). It appears in retrospect that the need for the solar satellites is probably smaller since even if a test was carried out behind the moon the fission fragments from the explosion would drift beyond the limb of the moon in less than a second, and the delayed γ rays from these fragments would remain detectable from earth-bound satellites. This means that the only major incentive for solar satellites would be to force the use of more complex shields for concealment.

You have heard of the Argus effect and the successful American tests to demonstrate it. In these tests, three weapons of about 1- to 2 kiloton yield were exploded at an altitude of about 300 miles. The fission fragments from the explosions produced electrons by radioactive decay, and these electrons were trapped in the earth's magnetic field where they remained for a very long time, of the order of 1 week, depending on the details of the altitude and energy of the charged decay particles. Such trapped particles can be detected by radiation counters carried in a satellite orbiting in this belt. This detection method is a very powerful one since a single satellite can patrol the whole region in which the charged particles are trapped and can sample the layer many times as the satellite circulates. For this reason the Working Group recommended that a single satellite for this purpose should be included in the control system. Unfortunately, this method is restricted since it covers only the specific region in which the trapping occurs, and

since this trapping is not possible in a large region over the north and south magnetic poles of the earth. Nevertheless, this method makes it possible to deny an otherwise very attractive region of space to a potential violator with only a single satellite and relatively simple instrumentation.

As mentioned earlier, the satellite-borne detectors will in general not provide coverage against tests carried out close to the earth but above the region included in the 1958 experts' report. This intermediate region cannot be neglected, in particular since considerably greater weights can be lifted to altitudes of, say, 1,000 miles than can be lifted to hundreds of thousands of miles. For this reason the group also worked on detection systems based on the ground.

Two of the systems involved observation of visible light which could be received on the earth after an explosion occurred at distances up to about 100,000 miles. These light signals would originate either from the explosion itself or from light produced in the upper atmosphere by the X-rays emitted by the device. It turns out that these two sources of light are independently useful since the direct light from the explosion would be very difficult to shield since the shield itself would become so hot that it would emit light also. On the other hand, the detection of light directly from the explosion is possible only in clear weather; for distant explosions, however, it would be extremely improbable that all control posts which could see the event were hidden by cloud cover.

The light produced by X-rays from the bomb in the upper atmosphere, however, is such that it will penetrate clouds without much loss, and it could be seen by instruments even in cloudy weather.

Other methods have to do with the influence of an explosion on the ionosphere, a layer about the earth which contains free electrical charges and which therefore affect the propagation of radio waves. The ionosphere makes radio communication beyond the horizon possible by reflecting radio waves of sufficiently long frequencies. The energy from a nuclear explosion at distances of many thousands of miles deposited in the ionosphere is sufficient to modify its properties so that the absorption and reflection of radio waves will change. This phenomenon can be made use of in two ways, (1) by the so-called ionospheric radar method, and (2) by the fact that the strength of radio signals from outer space received on the earth is changed; the second method was discussed earlier by the experts. Both of these methods had been studied experimentally in connection with the Orange and Teak shots of the Hardtack series, and also in the Argus experiments. These two methods are somewhat comparable in performance, although the ionospheric-radar method is probably more effective for weapons of smaller yields. When discussing these methods, we encountered, unexpectedly, opposition from the Russian delegates to including the ionospheric-radar method in even our technical evaluation. After very extensive discussions, in which the Russians tried to attack the technical validity of the method, it became clear that their opposition derived primarily from concern that the method might detect other phenomena of military interest, such as missiles passing through the ionosphere. We succeeded in reaching agreement on a joint technical evaluation of the ionospheric radar method, but not on the recommendation for incorporation into the proposals for consideration by the Conference.

I should now like to summarize the technical situation by showing a chart which gives the distances from the earth covered by these various methods (fig. 3, p. 48). You will note that we are discussing here a technically very complex system, and the complexity derives directly from our desire to obtain coverage at all altitudes starting from those considered by the experts up to hundreds of millions of miles. Because of the blind spots of satellite systems near the earth, the problem of achieving coverage near the earth can be resolved only by the use of the large number of methods we have discussed.

Despite the disagreement on the question of including the ionospheric radar method and the objection to the large number of methods, I believe in general the outcome of the Technical Working Group was quite satisfactory regarding the degree of agreement achieved. Also, I believe it did establish that detection is technically possible to the largest ranges considered, and that only complex shielding measures could counteract this capability and then only at very great distances.

Many questions concerning detection of explosions at high altitudes were not answered by the Technical Working Group, such as the problems involved in decisionmaking as a result of signals received, in procedures for using vehicles for launching satellites for this purpose, and in ascertaining that the satellites were operating properly. In addition, the system considered involves a large expenditure of money and of technical manpower, and will take considerable time

to achieve. Hence, the question as to what extent the control system is to be expanded to provide coverage for detecting explosions in space is still open.

[Attachment No. 1]

CHRONOLOGY

June 15, 1959: Terms of reference adopted by Conference on the Discontinuance of Nuclear Weapon Tests, plenary meeting 95 (GEN/DNT/56).
June 22, 1959: Technical Working Group I met for first of 20 formal meetings.
June 22–23, 1959: Agenda of TWG–I discussed.
June 23, 1959: Agenda adopted (GEN/DNT/HAT/PV.2).
July 10, 1959: Technical Working Group I adjourned.
July 10, 1959: Report of Technical Working Group accepted by the Conference during its 107th plenary meeting (GEN/DNT/109).

TERMS OF REFERENCE

"The Technical Working Group should assess the capabilities and limitations of possible techniques for the detection and identification of nuclear explosions at high altitudes (more than 30 to 50 kilometers) above the earth and, on the basis of the discussions and conclusions of the Geneva Conference of Experts, recommend techniques and instrumentation for consideration by the Conference for incorporation in the detection and identification system."

AGENDA

"1. Consideration of basic physical effects produced by high-altitude nuclear explosions in outer space and in the upper atmosphere.
"2. Detection and identification of high-altitude nuclear explosions by gamma radiation, neutrons, and other effects with the help of artificial earth satellites and other means.
"3. Detection and identification of high-altitude nuclear explosions by ionospheric phenomena, including the absorption of cosmic radio noise, by optical phenomena and other effects, which could be observed from the earth.
"4. Drafting of the report containing the recommendations concerning techniques and instrumentation for detection and identification of nuclear explosions at high altitudes."

Representative HOLIFIELD. Dr. Panofsky, if you will remain seated, we will call Dr. Brown forward.

Dr. Brown, will you come forward, please?

Both of you gentlemen worked on Technical Working Group II.[2]

STATEMENTS OF DR. HAROLD BROWN[3] AND DR. WOLFGANG H. PANOFSKY[3] ON TECHNICAL WORKING GROUP II

Dr. BROWN. Yes, we did. My understanding was that Dr. Panofsky would give the talk on Technical Working Group II, but we will both be prepared to answer questions.

Representative HOLIFIELD. You may proceed with the second item. This is new data that you are going to testify on at this time.

Dr. PANOFSKY. I should like to report on the work of Technical Working Group II, which met in Geneva in November and December 1959 to consider the problem of detection of underground nuclear explosions. As you know, the American delegation to the Conference on the Discontinuance of Nuclear Weapons Tests tried to reopen the question of control of underground nuclear explosions several times during late 1958 and 1959. On January 5, 1959, the American delegation tabled a working paper which presented the reasons they considered the underground-detection situation changed. The working paper (GEN/DNT/25) was based on new data obtained during the

[2] The Report of Technical Working Group II, will be found in app. 4, p. 599.
[3] See biographical sketches, pp. 6 and 35.

Hardtack series. It maintained (1) that the number of earthquakes equivalent to a given yield explosion which would occur naturally through the world was larger than had been assumed by the experts in 1958, and (2) that identification of earthquakes was more difficult than the experts had assumed, particularly by the so-called first motion method. During the following months, the American delegation frequently pressed for a reexamination of the technical situation. Agreement for such reexamination was finally reached, and Technical Working Group II, with Dr. James B. Fisk as chairman of the U.S. technical delegation, was convened on November 25, 1959.

Representative HOLIFIELD. At this point, for clarification in the record, the first working group worked on the results of one underground explosion, the so-called Rainier shot in Nevada.

Dr. PANOFSKY. No. The first Conference of Experts, which worked on the underground part of the work, had only the Rainier shot.

Representative HOLIFIELD. It had only the Rainier shot?

Dr. PANOFSKY. As its experimental backup material, plus, of course, general seismological experience and experience from chemical explosions.

Representative HOLIFIELD. Was Working Group I subsequently appointed?

Dr. PANOFSKY. Working Group I dealt with the high altitude problem only.

Representative HOLIFIELD. Working Group II gets back to the ground.

Dr. PANOFSKY. That was the reexamination of the technical situation on the detection of underground explosions.

Representative HOLIFIELD. How many underground nuclear explosions occurred in the Hardtack series and when did they occur?

Dr. PANOFSKY. The total number is five, but I do not have the dates here.

Representative HOLIFIELD. Just the year. Was it the fall of 1958?

Dr. BROWN. It was September and October 1958.[1]

Senator GORE. If I may, Mr. Chairman, whether either of the witnesses can answer this question, I do not know. I would like to submit it and if they can give me an answer, I would be glad to have it. The Hardtack series tests were conducted before the political conference in Geneva began on November 1. I was briefed.

Is that not correct?

Dr. BROWN. That is correct because the moratorium began the day the conference opened.

Senator GORE. As I have said here before, I was at the conference table on the first day on its convening. Before leaving Washington, I must say the very day before, I was briefed on the results of the Hardtack series of tests. Yet, upon arrival in Geneva I found the U.S. delegation proceeding upon the basis of the Rainier test with no recognition of the results of the Hardtack series. Is there a technical scientific explanation of this, or is the explanation found in the slowness of bureaucratic processes. I don't want to embroil you gentlemen in any area outside of your technical competency, but I just wonder if there is a technical reason why we found ourselves nego-

[1] The Hardtack II series included the following underground shots: Blanca (19 kilotons), Logan (5 kilotons), Neptune (90 tons), Tamplais (72 tons), and Evans (55 tons).

tiating for unrealistic goals about whose lack of realism our Government at certain levels was informed. Later, we proposed to recognize the Hardtack series results and then we were accused of backing up.

Now, is there a technical explanation of this? If so, give it to me. If not, you may proceed.

Dr. BROWN. I think I can give a partial explanation, Senator. The Hardtack II explosions took place in September and October of 1958, with most of the underground explosions of that series coming rather late in the middle of October and beyond.

The political conference which is still going on, the Conference on the Discontinuance of Nuclear Weapons Tests, convened on October 31. The data from the seismographs which were associated with the Hardtack II tests were processed and a number of very capable technical people continued to work on it through November, during which I believe you were in Geneva.

About November 23 or 25, about Thanksgiving, the people in the United States who were working on this data began to come up with some results and these results disturbed them a great deal. A preliminary statement about some of these results having to do with the size of seismic signals, which were smaller than had been expected for a given yield, and on the failure in many cases of the first motion criteria, was made available to the group in Geneva. Almost immediately thereafter, perhaps about a week thereafter, much of this data was brought over and some of the people who had been working on it, specifically, Dr. Romney, came over with it. This was talked about at some length in the delegation at the end of November and the first week in December. This was seen to be a fairly serious result; the degradation seen in the system seemed very serious.

The Conference recessed for Christmas about December 18—again I don't remember the exact date—and at that time it was felt important enough to evaluate this data so that a group of us convened in the United States to do so. They worked over the Christmas holiday and New Year holiday, and by January 25, this group, which was convened by the White House, had completed its deliberations and the so-called January 25 statement about the Hardtack II data was issued from the White House.

So there is roughly a period of about 8 or 10 weeks between the explosions and the issuance of this report. I would say that the reason for this delay was technical. It takes that long to do anything like a decent job of analyzing that mass of data. In fact, I would say that it was not until almost a year later that the data was really understood in what might be called a complete sense. The analysis has been going on from then until now.

What one has gotten is a series of successive approximations which get better and better. This first approximation was issued on January 5, about 2 months after the tests, and provided the basis for the actions of the U.S. delegation following that time.

So there was a delay. I would say that a fair part of it was inherent in the complexity and necessity for analyzing the technical data.

Senator GORE. The result was that the United States and the other governments at the conference took positions which were unrelated to the facts of the more recent tests. Speaking from strictly technical and procedural points, it would appear, would it not, the better part of wisdom to have awaited the results of tests before taking positions.

That is hindsight, however, and I will not ask you to go into it because that question is of a political nature. In any event we were accused of being insincere, of backing down, of trying to change the rules in the middle of the game, when, as a matter of fact, our Government was only trying to base its position and to persuade the conference to take a position based upon the most recent and thoroughly examined facts. Do you agree with that?

Dr. BROWN. I agree essentially with it. I think I should say that scientific facts always change and one must either try to anticipate their changing or else try to make allowance for the possible uncertainties.

Senator GORE. I did not think a scientific fact ever changed.

Dr. BROWN. I stand corrected. You are correct. I think our knowledge of nature changes as time goes on, and we may have mistaken beliefs about nature which are later corrected.

Senator GORE. I want the record to show that I scored one scientific fact.

Dr. BROWN. A home run.

Senator GORE. Thank you, Mr. Chairman.

Representative HOLIFIELD. As a matter of fact, additional scientific facts sometimes change to a degree the interpretation of facts which were known before; is that not true?

Dr. BROWN. Additional facts certainly change our understanding of things, yes, sir.

Senator GORE. But no facts ever change?

Dr. BROWN. That is philosophical.

Representative HOLIFIELD. Proceed, Dr. Panofsky.

Dr. PANOFSKY. The context in which this group worked was a very difficult one—much more so than in connection with Technical Working Group I on high-altitude detection. The Russian delegation were acquainted with our opinion that the capability of the Geneva system was less than had been believed, and they were concerned about our political objective in this working group. The Russian political leaders were presumably concerned with the possibility that as a result of the work of this group their delegation would be forced to sign a statement jointly with the United Kingdom and the United States that the situation concerning control was very much inferior to the situation believed to exist in 1958, and that as a consequence the chance of the negotiations' success would be impaired. Because of these worries, a substantial debate originated in the Conference concerning the terms of reference of the working group which was never fully resolved.

The United States wanted the terms of reference to include a reevaluation of the capability of the Geneva system based on new data; the Russians wanted to agree only to the presentation of the new data and of suggestions for the improvement of the system. The matter was finally compromised by having the terms of reference (attachment (1)[2]) include the discussion of the new data, suggestions for improving the system, and consideration of criteria by which suspected seismic events could be helpful for onsite inspection.

The question as to whether an evaluation of the system was to be included was bypassed, but it was the U.S. view that discussion of the

[2] See p. 87, attachment 1 to Dr. Panofsky's formal statement.

new data and studies when entered into the report of the working group would in fact constitute such an evaluation.

The working group began with the presentation by the United States of new data from the Hardtack series. Some of the data had already been released to the Conference by the January 5, 1959, paper, but a large amount of further detail was provided, including a set of the original seismograms. It became clear immediately that it would be very difficult to obtain agreement on any matter which would make it appear that the experts' control system was less powerful than had been believed, but that the U.S.S.R. delegation would press continually to obtain agreement on the various potential improvements to the system.

The U.S. delegation took the attitude that all facts, whether they made the system look better or worse, had to be considered. Progress under these circumstances was quite slow, particularly since many of the technical matters were very complex and hard to deal with in formal sessions; the Russian delegation would not agree until the last week of the meetings to our proposal for informal discussions among the scientists, although this had been very effective during the Conference of Experts and during Technical Working Group I.

There were extensive discussions on the problem of the number of earthquakes which would be expected worldwide to occur corresponding to a given yield nuclear explosion. The shots of the Hardtack series had indicated that the seismic signals produced were somewhat smaller than had been assumed by the experts; note that only one underground explosion had been fired prior to the 1958 Conference.

In deriving information from the Hardtack data about the number of earthquakes expected corresponding to a given yield nuclear explosion, the matter is greatly beclouded by the inaccuracy of our seismic information concerning earthwide occurrence of small earthquakes. Specifically, a body of information on the frequency of occurrence of small earthquakes exists only for California and New Zealand, and going from this information to worldwide estimates, and particularly to estimates for the Soviet Union, involves many uncertainties; these uncertainties are in fact considerably larger than changes in the situation created by the new data of the Hardtack series.

This issue is, therefore, not an important new consideration, and it now appears that probably its importance had been overemphasized in the United States. In discussing the matter with the Russians, however, we were faced with deliberate attempts by their delegation to force the estimates of the frequency of occurrence of earthquakes to a level well below the range of admitted uncertainties.

The issues involved here are highly technical and have to do with such problems as tying together the seismic scales of magnitude between different regions of the earth, and the question of how to handle a scattered group of data in a statistically valid way.

In the Russian presentations, attempts were made to reject data which made the estimated frequency of occurrence high while retaining data in the opposite direction. Also, the Russians introduced during the last sessions of the working group a new interpretation of the seismic magnitude scales which was totally unjustified, but which was repeated in their final report despite its rather obvious absurdity.

The discussions of this issue went on throughout the sessions of the working group and were not resolved. I consider this matter particularly unfortunate since the changes which are involved here according to our analysis are substantially smaller than the uncertainty with which the number of earthquakes equivalent to a given yield explosion can be determined without further research.

Representative HOLIFIELD. The Technical Working Group II report will be made part of the record (app. 4, p. 599).

Dr. PANOFSKY. In the experts' report, the only method treated in some detail for distinguishing earthquakes from explosions is that of so-called first motion.

I believe Dr. Brown has already explained to you the first motion method and I shall not go here into detail.

Representative HOLIFIELD. Could I ask you at this point, were the Hardtack series of test explosions all placed in the same type of earth, the so-called tuff earth?

Dr. PANOFSKY. Yes. With very similar conditions.

Dr. BROWN. They were almost all at about the same depth, too.

Representative HOLIFIELD. Would that indicate that you do not have the variability which might occur in the recordings if it had been in granite, let us say, or some other type of earth?

Dr. BROWN. That is right.

Representative HOLIFIELD. In order to have an international type of detection and interpretation of readings on the seismographs, would it not be necessary to have nuclear tests in different types of earth?

Dr. BROWN. It would certainly allow you to settle this question by having nuclear explosions in different kinds of rock. As I say, we expect it to be a factor of a few. A factor of a few may be important. On the other hand, it is not terribly important compared with a factor of a few hundred which you may be able to get other ways. Nevertheless, certainly at the time of the Conference of Experts, and even at the time of Technical Working Group II, factors of a few were considered important. I think they may still be fairly important.

Dr. PANOFSKY. These data were presented to the group at Geneva, and were attacked almost immediately by the Soviet delegations on a purely formalistic basis—that the instruments used by the American seismologists did not meet the specifications for such instruments set down by the experts. The assumption in this criticism was that it would be true a priori that the instruments proposed by the experts would be better than those in fact used. The relevant differences in the instruments were as follows: (1) The experts recommended that groups of 10 seismometers be used rather than the single instruments used in almost all the America stations. (2) A seismometer has a so-called natural period, which is the word of free swing of the seismometer after it is disturbed. The experts specified that the natural period should be 1 second (although as it turns out there is some ambiguity in the wording of the experts' report in this respect), and the American instruments had a natural period of about one-third second.

Any criticism based on the first of these differences is totally invalid, since the effect of using groups of 10 and more instruments rather than single ones had been measured by the American seismologists and found to be in general agreement with expectations. In the analysis of the data we had already corrected for the expected im-

provement of going from 1 to 10; thus there should have been no controversy on this point.

The second criticism also turned out to be invalid—not because the instruments were not in fact different from those specified by the experts (this is a matter of interpretation of the experts' report), but because we were able to show conclusively that the instruments used were actually better than those recommended by the experts in regard to the problems of distinguishing the direction of first motion in the presence of seismic noise.

The correctness of this technical fact was demonstrated during the meetings in two ways: First, by mathematical analyses carried out during the meetings; and second, by carrying out on very short notice an experiment in Oklahoma in which seismometers of the two characteristics to be compared were prepared, and very fortunately produced a clear record of an earthquake which occurred on December 9, 1959, during the time of the meetings and which showed the superiority of the instruments used during the Hardtack series in distinguishing the direction of fist motion.

Despite the fact that the technical situation had thus been clarified, the Russian delegation maintained their position by retaining in their final report the earlier criticism of our results as unworthy of consideration since the "correct" instruments were not used.

Representative HOLIFIELD. May I ask if that better instrument had been developed in the meantime, or did we have it prior to the Rainier shot?

Dr. PANOFSKY. It is the same instrument. The situation is as follows; it is a rather complex one. This so-called better instrument is what is known as a Benioff seismometer. It is the instrument which was used during the Rainier shot and also the Hardtack series. However, the Conference of Experts reported a description of the seismometer to be used. Its wording was such that the instrument whose data formed the basis of the evaluation of the Rainier and the Hardtack shots was apparently not covered by these specifications.

Representative HOLIFIELD. You mean in the description of the so-called correct instrument they did not properly describe the instrument that was used at Rainier, and the same instrument that was used at Rainier was used at Hardtack

Dr. PANOFSKY. That is correct It appears now that the wording of the experts' report is ambiguous in the English translation, but not in the Russian translation. In the Russian translation the way the wording actually appears it seems to indicate that the natural period must be 1 second, while in the American English version the wording is such that it specified what the maximum amplification of the instrument should be at a period of 1 second. This leaves open the question whether by that is menat that the amplification shall be a maximum of 1 second, or whether at 1 second the maximum amplification shall be a certain number.

In the Russian, however, this ambiguity did not exist and therefore the period of maximum response was 1 second. Therefore, in their interpretation, which we could not disagree with, the experts' report actually described an instrument which had not been used at all in either of these series. Therefore, this nonexisting instrument was assumed by the Russians to be one which was a priori better than the

instrument which was used in the actual series in spite of the fact that it did not exist. I hope I made myself clear.

Representative HOLIFIELD. I understand, I think, what you said. You have in effect said that the difference in the interpretation of the Russian language and the English language resulted in the fact that the Russian language described an instrument which did not exist. Nevertheless, it was an attempt to describe the instrument which was used in the Rainier shot, and in the Hardtack shots. Therefore, there was no improvement or weakening or strengthening of the original instrument. And so, therefore, your readings came from the same type instrument in the Hardtack series as in the Rainier series, but you had more of them in the Hardtack series.

Dr. PANOFSKY. Yes.

Representative HOLIFIELD. Therefore, you had more data to analyze and to digest, and you came up with a different answer on the five-shot test series than you did on the one-shot test series. Have I analyzed that about right?

Dr. PANOFSKY. That is precisely correct. It was, of course, the intent of the experts to word the description broadly enough to include the instrument actually used in the Rainier shot and this is evident by reading the verbatim transcripts of the Experts' Conference. On the other hand, it was attempted to leave some latitude in adjusting this physical parameter to the optimum value. It was unfortunate that this particular matter came up in this way. The technical situation is precisely as you described. Therefore, the Russians chose to interpret the experts' description to be a fixed natural period of 1 second despite the fact that the entire body of experience was based on the natural period of approximately one-third of that amount.

Representative HOLIFIELD. Let me ask you this. In the science of seismology what type of instruments have the Russians used over the years as compared to the type we have used over the years? Have they been substantially the same or have they been of substantial difference?

Dr. PANOFSKY. I am not a seismologist and if you wish further details, Doctor Romney should answer that.

Representative HOLIFIELD. We will have that later.

Dr. PANOFSKY. I can give some answer. They have an instrument which they discussed as being closer to their interpretation of the experts' version which did have a period of about 1 second.

Representative HOLIFIELD. Did they show those instruments to you?

Dr. PANOFSKY. They described them and described their essential characteristics. In fact, this comparison test at Oklahoma was based on having us take the description of their instrument, mocking it up—one can do this—and comparing it with the other instrument and running both of them together. We actually took an instrument of precisely those characteristics which the Russians attributed to their own instrument; by duplicating its characteristics in this Oklahoma test, we were able to show quite conclusively that the shorter period instrument gave a much more satisfactory performance in regards to reading the direction of first motion in the presence of noise.

Representative HOLIFIELD. Did they furnish a sample of their instrument or did you make the instrument according to their description without seeing their instruments?

Dr. PANOFSKY. We made the instrument according to their description. It is not a matter of description so much. It turns out that a basic graph which they did furnish to us describes in all essential features the performance from the point of view of this particular problem.

Representative HOLIFIELD. Since you made that test of the two machines, have they acknowledged the validity of the one-third second machine?

Dr. PANOFSKY. No. This was actually a very amusing situation. After we introduced these items, the Russians inquired whether we would like to introduce our machine as a possible improvement to the system, despite the fact that it had been used all the way along, because of the fact that legally speaking in terms of their interpretation of the wording, the 1-second instrument was the one of the experts. We agreed that if that would be more pleasing to them, we would be delighted to introduce our instrument as an improvement, but in that case one should realize that our Hardtack data already involved the use of the improved system.

So that was the way the matter was left, but in the final report they still reiterated the fact that our conclusions were of no value because of the incorrect instruments having been used. As you can see, there were many places where legal training rather than technical training would have been of great advantage to us.

It is indicative of the problems encountered during these meetings that it was not initially clear within the U.S. delegation itself when the Soviet delegation raised these objections whether they were in fact valid. It was only the analysis carried out during the sessions and the successful experiment so rapidly carried out that demonstrated the validity of the American instrument readings. I note here that in their final report the United Kingdom delegation stated their complete agreement with the American technical points.

The Russians also criticized the U.S. delegation on the ground that our "new data" presented during the Working Group did not differ significantly from the material introduced to the Conference in January 1959. This criticism is to some extent valid since, as the example given above indicates, not a great deal of further analysis of the data was carried out in 1959.

The next new information introduced concerned the problem of decoupling which is possible by detonating an underground explosion in a large hole. At the time of the meetings of the Working Group there were only preliminary experimental data on this point from work carried out by the British and presented by their delegation. Our estimates consisted of a combination of theoretical calculations and measurements made during the earlier Rainier explosion, and this made a very convincing story that seismic signals could be reduced by about a factor of 300 relative to the conditions pertaining at the Nevada test explosions. These calculations and data were introduced by our delegation, and were first attacked on technical grounds by the Soviet group. After detailed discussion indicated that the U.S. calculations very likely were correct, the Russians changed their position from technical attacks on our conclusions to the statements that they were "premature."

Material was introduced by both the United States and Soviet delegations on many suggestions for improvement of the experts'

system and for improved methods of analysis of seismic signals which could be recorded. There was very little disagreement on these; after discussion of the methods, the results were agreed upon by all delegations.

The technical situation here is very unfortunate. There exist a large number of potentially useful sources of seismic information which have bearing on the problem of distinguishing earthquakes from explosions. They are all based either on theory—which, in view of the complexity of the earth's crust, might not be in accordance with observations—or on very incomplete experimental observation.

Among such methods are the use of long-period waves and their interpretation, reduction of noise by burying seismographs in deep holes, and improvement of the operating characteristics of seismic equipment in general. I shall not discuss these in detail since they will be dealt with in further testimony during these hearings.

The principal problem is that these methods are potentially useful, but within the present state of knowledge there is no way of evaluating quantitatively how useful they could be nor is it possible to write specific recommendations under which they could be used by unskilled operators. In the hands of skilled seismologists, even at present the use of some of these methods might very well improve the judgment which can be exercised in distinguishing earthquakes from explosions.

Many of the improvements possible had been discussed previously and are described in the publicly released Berkner report.* In addition to the possible methods introduced at the Technical Working Group, the Berkner Panel also considered the possibility of introducing unmanned stations as "slaves" of the control posts. These stations might improve the system performance significantly, but at the same time add a great deal to the cost and complexity, in particular in regard to the communication networks required.

In addition, such stations would require more personnel at the control posts and access of these personnel to the unmanned stations. None of these factors had been evaluated with sufficient care prior to the work of Technical Working Group II to enable us to discuss them in the negotiations.

The next major topic discussed was the question of "criteria for initiating onsite inspection." Whenever a seismic event is recorded on the instruments of the control system, the decision must be made whether it is eligible for onsite inspection. The system designed by the experts has enough sensitivity that it would detect but not identify roughly 15,000 earthquakes a year on a worldwide basis. Note again that this estimate is uncertain by a factor of about two or possibly more. Although all parties have agreed in principle that a certain number selected from these events should be eligible, neither the mechanism for making the selection nor the number of events allowed to be inspected has been agreed upon. It is clearly desirable first to reduce the number of eligible events by using the seismic information produced by the control system itself.

Representative HOLIFIELD. I think this last statement that you made is very important because many times we pick up a newspaper and you find a headline which says that the Russians have made another concession. They have conceded this and they have now adopted the American position, and so forth. Here clearly are two very important and vital areas to any kind of agreement and appar-

*See app. 5, p. 643.

ently there is no concession on their part or no agreement on the part of the negotiators as to these particular areas.

Dr. PANOFSKY. That is correct. As I will discuss later on, there are certain concessions and areas of agreement, but there is still a great deal of work to be done. There are still a large number of items where agreement is necessary, which does not exist as yet.

Representative HOLIFIELD. I heard an example once described in this manner. They agreed on the color of the automobile but they had not agreed on how many wheels, or whether there should be an engine in it yet.

Dr. PANOFSKY. I must say that this analogy is probably not too applicable here. I believe that actually the concessions which have been made are significant. I only would like to point out that there are a large number of items which are left which may very well turn out to be very contentious.

Representative HOLIFIELD. Maybe they have not agreed on the color either. You may proceed.

Dr. PANOFSKY. Depending on the accuracy to which the instruments of the control posts have been calibrated, a detected event could be located in an area between 50 and 300 square miles. Eligibility for inspection is presumably confined to such an area. If a certain number of detected events can be definitely identified as earthquakes, they would no longer be eligible for inspection. We have called criteria which definitely identify a seismic event as an earthquake "negative" criteria.

If a seismic event can be identified with a high degree of confidence as an explosion, then it could in principle be made mandatory that an onsite inspection be made. I shall call such criteria "positive." The U.S. delegation formulated a rather complex set of negative criteria which would identify a certain number of seismic events as earthquakes but would leave a large fraction of the remaining events eligible for inspection.

In arriving at these criteria, the U.S. delegation assumed that these criteria must not be so stringent that in case a nuclear explosion did occur more than half of such explosions would not lose their eligibility for inspection by virtue of these criteria. Based on the knowledge available to the delegation, these criteria turned out not to be very powerful, and in particular almost all seismic events of small magnitudes would remain eligible.

The U.S. delegation was unable to formulate any positive criteria, beyond agreeing with the other delegations that seismic events originating in those areas which are usually considered "aseismic" (in which earthquakes, to our knowledge, do not occur) should be subject to inspection. The Soviet delegation took the position that it was the duty of the working group to formulate criteria which would remove eligibility for inspection from all but a very small fraction of seismic events.

Senator GORE. That is a significant statement, it seems to me, Doctor. Why, in your opinion, or how, in your opinion, could that be the duty of a scientific group, that it was the duty of the working group to formulate criteria which would remove eligibility from events? How could the working group scientifically have that duty?

Dr. PANOFSKY. The position taken was that dealing with 15,000 eligible events on a nonscientific basis, making a selection on some

arbitrary basis, was not practical. Hence, scientific methods must be found to make this reduction possible. This was the attitude which they took.

Senator GORE. In other words, you are telling us the attitude they did take and not why they took it.

Dr. PANOFSKY. That is correct.

Senator GORE. That is fine.

Dr. PANOFSKY. I will continue here with my testimony which I think will make the situation somewhat clearer.

The fact that the question of whether this can be done is a technical rather than a political one did not change their position. In several interesting exchanges, the Russians insisted that, if science was required to find solutions to a problem, then such a solution would necessarily be found.

This was a rather interesting exchange. The point which they were trying to make was that science had been able to solve many things which were considered impossible of solution up to that moment. So why shouldn't they do it again if political necessity demands it? Our position was that this may or may not be so, but we had to base our discussion on the present state of affairs. The discussion went back and forth on this philosophical point: that science can always be made to do what it was supposed to do, and we said it could not.

Senator GORE. In other words, you were assuming the position of a scientific delegation, and refusing to be drawn into political decisions?

Dr. PANOFSKY. We would say that the scientific situation is a certain situation, and these are the conclusions, and that the potential of science is limited by our knowledge at the present time. Whatever political demands were made on science did not change the present scientific situation: that decisions of this kind involved necessarily a mixture of scientific and political considerations, but that is no reason why the science itself should be subjected to change.

Senator GORE. You mean the ultimate decision as between countries would be a mixture of scientific and political considerations?

Dr. PANOFSKY. Certainly.

Senator GORE. But the political considerations were not the prerogative of a scientific conference.

Dr. PANOFSKY. That is correct. I will continue with my formal testimony.

The Soviet delegation introduced a set of criteria which would in fact remove eligibility for inspection from a very large number of seismic events, including the explosions of the Hardtack series. The Russians did not include these in their final report, and we believe that they were not intended to be taken as a serious proposal.

Senator GORE. It seems to me, Mr. Chairman, that the doctor is here giving some important testimony. Do I correctly understand the concluding sentences of the paragraph which you have just read to say that if the Russian criteria had been followed even our own Hardtack series of tests would not have been subject to inspection?

Dr. PANOFSKY. That is correct.

The Russians introduced a series of criteria which I believe were prepared rather hastily by their delegation, and if interpreted rigorously would have led to the result which is indicated, namely, that the Hardtack series shots would have been interpreted as earthquakes and eligibility for inspection would have been removed. However,

as I said before, I think it was the Russian intent to have these criteria only be made the basis of discussion; but they did not repeat them in their formal final report. Therefore, we believe that they did not intend them to be a serious scientific technical proposal.

Representative HOLIFIELD. Will the gentleman yield?

Senator GORE. Yes; I have finished.

Representative HOLIFIELD. I was just conferring with the staff here and I think we should put in the record at this point the respective power of the five Hardtack shots. As I remember, the Blanca shot was about 19 kilotons.

Dr. BROWN. That is right.

Representative HOLIFIELD. And the others I think were less. Do you have them at hand?

Dr. BROWN. I can tell you roughly what they are. There was one about 5 and there were several below 1 kiloton. The interesting point is that even the 5- and the 19-kiloton explosions would by the originally presented Soviet criteria have been identified as earthquakes, because they all produced some rarefactions.

Representative HOLIFIELD. In addition to refusing to consider the 19-kiloton Blanca shot, they were perfectly willing to rest their case on the 1.7 Rainier shot.

Dr. BROWN. Yes. As Professor Panofsky pointed out, they were not really interested in a technically valid control system. They know there would not be any clandestine nuclear explosions in the West. They want to minimize access to their country, and, therefore, it does not matter to their country if a system detects nuclear explosions, or not. The same would apply if they intended to cheat, which I am not suggesting they would.

Representative HOLIFIELD. There is no consistency in accepting the data of the September 1957 Rainier shot of 1.7 and then asking that shots of the nature of 19 kilotons be excluded by the criteria they suggest.

Dr. PANOFSKY. I think their position is not quite as inconsistent as that, because since they did not acknowledge the validity of our measurements taken during the Hardtack series, therefore, the conclusion that their criteria would have made all our Hardtack shots ineligible for inspection is based on our measurements. Therefore, if they took the legalistic position that because of the wrong instrument situation, or whatever one would consider, that our measurements had no bearing on the situation, then their position is not quite as inconsistent as that.

This conclusion which I have written here that the Soviet criteria would have removed eligibility from all the Hardtack series explosions, is our conclusion and is based on our technical results.

Shall I continue?

Representative HOLIFIELD. You may proceed, sir.

Dr. PANOFSKY. The difficult question which faced us during the discussions and which faces us now is how to make use of the large body of seismic information called auxiliary or diagnostic aids, which might be useful but which cannot now be codified into specific criteria and whose effectiveness we cannot now evaluate.

If choice of events to be inspected is left to one of the parties carrying out the inspection on the territory of another, then this auxiliary information might be helpful to an as yet unknown extent. At the same time, as knowledge progresses, some of this information might

grow into useful formal criteria. Beyond acknowledging this state of affairs, however, there was no way in which the U.S. delegation could responsibly introduce the use of these aids into the discussion.

Since no agreement was reached on the basic technical data, it was clearly impossible to reach agreement on the formulation of the criteria for on-site inspection, despite the prominence of this item in our list of agenda.

Representative HOLIFIELD. At this point, Dr. Panofsky, this brings up again the evaluation on the part of members of the Congress and the public as to what areas of agreement have been reached in these particular 17 months of conferences. Here is a plain statement that this area here has not been agreed to.

Senator GORE. Mr. Chairman, as you said a few moments ago, we have repeatedly read of so-called Russian concessions which later turned out to be more apparent than real. The most notable one was in a joint communique of March 19, in which the world was misled into thinking that the Russians had accepted President Eisenhower's proposal, when in fact they had not. Since this was a joint communique, I suggest that Ambassador Wadsworth, who I understand is to arrive in Washington today, be invited to appear before the committee and explain to us the circumstances of this joint communique which misled our country and the people of all countries.

Representative HOLIFIELD. Your suggestion seems to be a good one to the Chair. I am not aware of whether this would be embarrassing to our Government to have its negotiator appear before a congressional committee at this point in the negotiations, but if it is the committee's will, we can explore this and if it would not affect the negotiations deleteriously, I would certainly be willing to extend an invitation at the direction of the committee.

Senator GORE. If the administration should prefer it be an executive session, I would have no objection, although I do think that the American people are entitled to the information.

Representative WESTLAND. Mr. Chairman?

Representative HOLIFIELD. Mr. Westland.

Representative WESTLAND. Dr. Panofsky, do I understand that your recommendation, which seems to be in the next to the last paragraph, seems to be that only tests giving rise to signals above 4.75 would be banned and that 20 to 30 inspections would satisfy our requirements for adequate controls?

Dr. PANOFSKY. I have not gotten to that point in my testimony.

Representative HOLIFIELD. Will you proceed, please.

Dr. PANOFSKY. With the exception of the suggested improvements to the detection system, the working group ended with a disagreed report—really a letter of transmittal with three reports appended.

I should like to comment briefly on the relation of the results of Technical Working Group II to the American "threshold proposal" to the Conference and to the recent Russian proposal to accept the threshold in combination with a moratorium on underground nuclear tests. The U.S. proposal attempts to relate the proposed test ban to the capability of the system for detection and identification. This is done by defining a threshold in terms of a seismic magnitude such that only tests giving rise to signals above such a magnitude would be banned. The magnitude of 4.75 was chosen, which corresponds to the seismic signal produced by about a 20-kiloton nuclear explosion,

closely tamped, and fired under the conditions of the Nevada test site. At this value the total number of seismic events remaining eligible for inspection would be small, so that it is believed that 20 to 30 actual inspections a year in the Soviet Union might be both acceptable to the U.S.S.R. and satisfy our requirements for adequate control. At the same time, the threshold is set at such a high level that the incentive for carrying out large-hole decoupling would be reduced since holes which would provide decoupling for explosions much larger than 20 kilotons might have to be very large engineering enterprises.

The principal concession made by the Soviet Union in accepting the threshold concept is the implied acknowledgment that there do exist substantial problems in the detection and identification of underground explosions; their acceptance is indicative of their willingness to negotiate on our technical terms. Despite the disagreement in Technical Working Group II, its work did serve the purpose of conveying, apparently convincingly, the technical problems involved in detection of underground explosions, and did make it possible to carry out further negotiations on a political rather than a technical basis.

Representative WESTLAND. Now, Doctor, could you answer my question.

Dr. PANOFSKY. I am sorry, could you repeat your question, please?

Representative WESTLAND. Very well. I asked whether or not it is your recommendation that only tests that give rise to signals above 4.75 be banned, and that 20 or 30 inspections would satisfy our requirements for adequate control.

Dr. PANOFSKY. This is the official U.S. position, but not exactly in your terms.

Representative WESTLAND. I asked if it is yours.

Dr. PANOFSKY. Whether it is my recommendation?

Representative WESTLAND. Yes.

Dr. PANOFSKY. I would prefer not to answer this question.

Representative WESTLAND. That is one of the reasons we have you gentlemen here.

Dr. PANOFSKY. The reason I would prefer not to answer this question, sir, is because it is a matter of political judgment; namely, is a given fraction of events to be inspected an adequate deterrent to keep the other fellow from violating the treaty? This is a matter of political judgment which I feel is not one where we have any more to contribute than any other citizen.

Representative WESTLAND. Maybe I have not understood everything you two have been saying here. I doubt if many people in the room have unless they are men of scientific mind. What I have understood, or what I have acquired out of this is the fact that you cannot detect underground explosions below this threshold if you have proper decoupling and proper shielding, and that explosions in areas lower than 19 kilotons can be of very considerable benefit to furtherance of nuclear weapons.

Am I wrong? Have I come to the wrong conclusion?

Dr. PANOFSKY. Technically your conclusion is approximately correct. It is true that you can detect nuclear explosions much smaller than that, but that the chance of identifying them correctly is low. Therefore, to turn the situation around, the chance of not being caught

for a violator is good. On the other hand, before one bases a recommendation on that conclusion, one has to take into account matters which are not part of either my testimony or my competence: (1) the importance of continuing weapons development in this area, (2) the mechanical effort required on the part of a violator to carry out this violation, (3) what this effort would do for him if deployed in other areas, and (4) the evaluation of what chance of detection would in fact be an adequate deterrent to another nation. It is these questions which I feel are entirely a matter of political judgment, which combined with the general importance of proceeding with this treaty, must be taken into account before a valid assessment is formed. It is for this reason that I would prefer not to give an answer to this particular question.

Representative HOLIFIELD. Doctor, if I might just say in the opening statement of the purpose of this hearing, we clearly stated that we wanted the testimony to be on the technical capabilities of the system.

Dr. PANOFSKY. Yes.

Representative HOLIFIELD. I really think, as Senator Gore said this morning, that you are to be complimented on confining your answers and your statement to the technical points. I think I am as much interested in the political decisions as any person at the table, but I do not believe that this series of hearings is for the purpose of going into the philosophical and into the political statements. What we want to do in this set of hearings, if possible, is to lay the technical facts on the table and let each individual on the committee and then the audience and among the witnesses draw their own conclusions from those facts. I hope that the committee agrees with the purpose and that we will not push any of the witnesses into making a conclusive statement based on philosophy or policy matters.

We tried to follow this same method in our radiation hearings and I think it came out very successfully, because we did place on the record a great many facts and the committee did not take the position of passing judgment on it. We left it for the American people to decide.

Dr. BROWN. I think that part of Mr. Westland's question does have a technical answer, and I think Doctor Panofsky responded to it. Perhaps I can also. The proposal of early February—I believe it was February 11—made by the United States that tests of seismic magnitude of 4.75 and above be banned for underground shots, and that 20-odd inspections a year would then be satisfactory to monitor that agreement, is, I think, an essentially technical conclusion. I think for that magnitude the detection system will detect and locate and identify a large enough fraction of the events, so that you could inspect a large enough fraction with the 20 inspections a year per country to make a good system.

Representative HOSMER. Dr. Brown, just a moment. Your statement presupposes that you have an inspection procedure that itself is adequate, and that is the subject we have not gotten into at all.

Dr. BROWN. That is correct. Nevertheless, if you were to inspect half of the unidentified events, that gives you some capability. I don't say this gives you a perfect capability. Below 4.75 magnitude the situation is quite different. Of course, such an arrangement does not purport to control explosions below that level at all.

As I understand the Soviet so-called concession, it is that they will agree to a treaty above 4.75 magnitude with some number of inspections, providing that there is a moratorium, that is not a formal agreement but an agreement nonetheless, not to have nuclear explosions below that threshold. That, as I think has been clear from the testimony so far, but may be elucidated further, is not in any sense control in the same way.

Representative HOSMER. May I ask this technical question?

Would a violation show up any different on the detection equipment if it was a violation of a treaty than if it were of a moratorium on explosions under 4.75?

Dr. BROWN. They would be detected or undetected in the same way. It is a somewhat formal arrangement and the Soviets have made acceptance of the threshold, as I understand it, conditional on the acceptance of the moratorium below the 4.75.

Representative HOSMER. In this connection, are we talking about the old Richter scale or is it something different?

Dr. BROWN. What we are talking about here is a unified magnitude, modified to use seismographs from about 20° out to about 90° of arc, measured on the surface of the earth.

I believe that this magnitude was set up purely for this purpose, but it corresponds pretty closely to the Richter scale.

Representative WESTLAND. Suppose you invented a machine that could register under 4.75 and you detect something that is suspicious and then you are faced with this treaty which specifies a 4.75 figure, and the Russians say, "No; you can't look"? Then what do you do?

Dr. BROWN. It is not clear what the situation is below 4.75. I do not know how the negotiations stand. I am not associated with the delegation any more.

Representative WESTLAND. You believe that your detection equipment will continue to improve?

Dr. BROWN. Detection can improve and, of course, methods of concealment can also improve. I think one could modify or adjust such a threshold as a function of time. It will naturally be adjustable as a function of time.

Representative BATES. Doctor, how many explosions or earthquakes would you expect in this magnitude in the course of a year?

Dr. PANOFSKY. In the United States?

Dr. BROWN. Above 4.75?

Representative BATES. Yes.

Dr. BROWN. I believe Dr. Romney will cover that in the next talk. My memory says that there will be the order of 50 or fewer per year of this magnitude in the Soviet Union.

Representative BATES. Once these are reported and the inspections follow, then after you hit the figure of 20 or 30, that is the end of the inspections, anybody can go ahead and set off anything they want.

Dr. BROWN. One would presumably arrange the inspections so that you did not run out of your quota before the end of the year. I think that would be fairly important.

Representative BATES. That is the point I want to make, make sure you keep a few in abeyance and hope you get the right ones.

Dr. BROWN. That would be the idea.

Senator PASTORE. A matter that concerns me arose during the development of this question which was put to the witness by Mr.

Westland, to the effect that it is the American position that 20 to 30 inspections would be adequate. I think Dr. Brown substantiated that technically that number of tests is considered to be adequate. Do I understand correctly?

Dr. BROWN. It would be adequate to inspect a large fraction of the unidentified events that involve a magnitude of 4.75 or above.

Senator PASTORE. A question was put to Dr. Panofsky whether or not he agreed with this position and he said he preferred not to answer. Do you disagree with it? Do I make my question clear? When the question was put to you whether or not you agreed technically that the American policy as enunciated in your paper would be adequate, you said, "I prefer not to answer that question." Now, I am asking the question, why do you prefer not to answer it? Do you disagree with it?

Dr. PANOFSKY. The reason I prefer not to answer it is essentially the point given by Mr. Holifield; namely, that the establishment of this policy does not only involve whether things are identifiable or not, but whether this is a large enough fraction. One could, for instance, insist that all identified events should be inspected. One might be satisfied with only one or two a year out of this. This is not a technical thing.

Senator PASTORE. The question I am getting at is this: I am wondering whether or not we are being confronted here with a paradoxical situation. If we are taking a calculated risk, if 20 to 30 inspections may not be sufficient on the big shots, why are we so careful and cautious in making an agreement below the small recordation below 4.75? It seems to me where we are willing to go we are taking a chance anyway, according to your testimony.

Dr. PANOFSKY. That is right. I agree completely that in any agreement which we will make there will be elements of risk.

Senator PASTORE. In other words, even what we are willing to agree to accept, if the Russians would accept it, our position involves a calculated risk.

Dr. PANOFSKY. Certainly. Let me possibly make the point clearer. The problem of adequate control of nuclear explosions or any arms limitation process will always involve an element of risk. If you demand that you have a control system which is certain on technical grounds alone, then there is no such a thing. Therefore, whether a control system is adequate means whether you are willing to accept this risk inherent in that particular degree of control, and involves among other things evaluation of the risk of not having a control system, of not proceeding with disarmament measures and arms limitation questions.

Senator PASTORE. Which is a political question.

Dr. PANOFSKY. Which is a political thing. It is for that reason I would prefer not to evaluate whether the degree of risk which is implied here in this number is a correct or not a correct assessment of the degree of risk which is acceptable.

Senator PASTORE. Would you care to venture at this time a guess as to the degree of risk involved even with the plan we consider to be satisfactory?

Dr. PANOFSKY. By that you mean what is the chance of a violator getting caught?

Senator PASTORE. Yes, or a violator actually setting off a clandestine shot without being known or identified or detected.

Dr. PANOFSKY. As a technical situation it is as follows: At magnitude 4.75, and using the presently agreed distribution of control posts, the chances are about 50–50 that a 20-kiloton explosion would get identified as an earthquake or remain eligible for inspection.

Representative HOLIFIELD. Providing it is not decoupled and providing it is in tuff.

Dr. PANOFSKY. Yes.

Let me repeat the statement: At 4.75, if a 20-kiloton nuclear explosion were set off under the same condition of Rainier coupling, then there is about a 50–50 chance that within the control system as envisioned——

Representative HOSMER. Does that include the inspection part of it?

Dr. PANOFSKY. No. It would remain inspectable. The number of events which would be remaining according to Dr. Brown, are about 50 to 100, and therefore 20 percent of those might be 20. So this means that you have, then, 20 percent times a half of 10 percent total chance of having an onsite inspection. So the overall chance of getting caught is then the product of that 10 percent times whatever one evaluates the chance of success of an on-site inspection to be.

Senator GORE. That means 9 to 1. Nine to one if one assumes the chance of on-site inspection to be 100 percent. These are rough numbers.

Senator PASTORE. From a technical point of view the paradox that I am talking about is this: We are being extremely cautious, and justifiably so, with reference to making a commitment as to these shots that will take place and record below the threshold of 4.75. We are saying continuously to the American people. and we are saying to the free world, that America cannot accept that kind of a proposal unless we have an airtight adequate system of inspection which we have not yet developed—or convinced the Russians that we have developed—on these smaller shots that will not record up to the point of 4.75. Yet you are saying here today that no matter what we agree to, we take a terrific calculated risk. I notice the other gentleman is wincing in his chair. You are nodding your head in the affirmative and he is denying it. That is the thing that is puzzling here. You are agreeing with everything I say and your colleague is becoming very uncomfortable because you are agreeing. That is the thing that is disturbing me. Could we get that cleared up?

Dr. BROWN. Sir, what makes me uncomfortable is the implication which I am sure you didn't mean, but I may have mistakenly gotten, that all risks are the same. I think that a system which has even a 10 percent chance of being effective—and I am not saying this is the correct figure above 4.75, but maybe it is 10 percent—is one thing. A system which has zero chance of detecting a clandestine explosion because the clandestine explosion can be decoupled to the point where it is not detectable at all is quite a different matter. They are both risks.

Senator PASTORE. You are talking in terms of degrees.

Dr. BROWN. That is right. But it is a large degree.

Senator GORE. Have you finished?

Senator PASTORE. For the time being, yes. I may come back to the question.

Senator GORE. I would like to know if I understand the percentages and degree of risk with the decoupling or without decoupling.

Dr. PANOFSKY. This is the degree of risk for a 20-kiloton explosion without decoupling. There is, however, this remark which I have already mentioned, namely, that since the size of a required decoupling hole increases with the size of shot you are willing to decouple, therefore the payoff in decoupling is a great deal less when you are talking about a threshold as high as this, than the payoff would be if one were talking about a smaller number.

To answer your question, I have indicated the approximate degree of risk which there would be in a 20 kiloton nondecoupled shot fired under the same conditions prevailing at the various shots in the Nevada test site.

Senator GORE. Assuming that a country would be willing to undertake the expense of the geometrically larger hole, if that is a way of expressing it, for the decoupling of a larger explosion. I understand that you say this would become expensive and that the country might not wish to do it because of that. But assuming that the testing of some particular weapon was so desired that a country was willing to undergo the expense of decoupling a larger weapon, say 50 kilotons, to the maximum of decoupling, what would be the percentage of detection then? I mean percentage risk of undetection.

Dr. PANOFSKY. If a country would construct a hole such that the 50-kiloton shot was decoupled by the factor of 300 then it would look only like two-tenths of a kiloton, and then his chance of even being located would be very, very small. So I think the answer to your question is zero.

Senator GORE. Zero?

Dr. PANOFSKY. That means if he was willing to go to the expense of fully decoupling, or building a hole for 50 kilotons, which is a very large hole, then within the present system his chance of getting caught is negligible.

Senator GORE. You have told us, if I correctly understand your testimony, that without decoupling, according to the proposal of February 11, there would be a 10 percent chance of detecting a 20-kiloton detonation uncoupled. If decoupling is maximized there would be practically no chance at all of detection.

Dr. PANOFSKY. Well, I would agree with your statement excepting that there would be a 10 percent chance that a 20-kiloton explosion would actually be inspected.

Senator GORE. I stated that.

Dr. PANOFSKY. The chance, with an adequate cavity for giving full decoupling, for 50 kiloton to be even detected would be negligible. I agree with your statement.

Senator GORE. I have one further question. It may be one, Mr. Chairman, which these gentlemen may be able to answer or may not be able or may not wish to answer.

About a year ago, I believe, Prime Minister Macmillan went to the Soviet Union and allegedly suggested the quota system for inspection. Theretofore the United States and Great Britain had been insisting that there be free inspection of suspicious events. Our Government was practically pulling its hair out because of Prime Minister Macmillan's suggestion.

Are we not now suggesting or have we not accepted and suggested ourselves a politically determined quota? If not politically determined, an arbitrary quota?

Dr. BROWN. It is somewhat arbitrary. However, I think that even before a year ago the United States was suggesting that it might not be necessary to inspect all unidentified events. So that so long as this remained a substantial percentage of the unidentified events I would say there was no logical change in the U.S. position.

Senator GORE. What do you mean by logical change?

Dr. BROWN. What I mean by that, sir, is that the quota, if it is a percentage of the unidentified events, is a technically determined point.

Senator GORE. What percentage would 20 be of the natural events?

Dr. BROWN. Above 4.75?

Senator GORE. Yes.

Representative HOLIFIELD. I wonder if at this time, Senator, I could read into the record a portion of this transcript which is our official national position on this.

Senator GORE. Surely.

Representative HOLIFIELD. It was given by Ambassador Wadsworth at Geneva and is under date of February 11.* It has been released today. I will read it as follows. It is not too long. This is Mr. Wadsworth speaking at the Conference:

> As I said earlier, the specific threshold magnitude to be adopted under this approach would depend in large part on the level of inspection that the other delegations were prepared to accept. However, as a specific proposal under this approach my delegation is authorized to suggest the adoption of a threshold of magnitude 4.75 on the unified magnitude scale in common use by seismologists. Thus, depending on whether we are able to reach an agreement on criteria, either all seismic events of magnitude greater than 4.75 located by the system would be eligible for inspection, or alternatively only those events above such magnitude left unidentified after the application of the U.S. criteria would be eligible for inspection. In the former case we would propose that 20 percent of all events located by the system would be subject to inspection.
>
> In the latter case we would propose that 30 percent of unidentified events be inspected. On the basis of the best estimates provided by U.S. scientists and assuming the existence of control posts initially only on the territories of the 3 original parties, it appears that either formula applied to events of magnitude 4.75 or greater would result in about 20 inspections in the Soviet Union in the average year.
>
> At this point I should perhaps interpolate that our scientists believe that roughly speaking about 100 seismic events above a threshold magnitude 4.75 are likely to occur each year in the area of the Soviet Union and that about 70 of them are likely to remain unidentified by the original control net. Again, speaking very roughly, we believe that the number of comparable events occurring on the territories of the United States and the United Kingdom taken together would be approximately the same.
>
> Once more I wish to make clear that the choice of which events were to be inspected from among those eligible would not, of course, be subject to any veto by the party on whose territory the inspection were to take place. This choice would be left to the other side.

I think that is all. What I wanted to establish was that the figures that are contained in our official position are 100 overall events in the Soviet Union, or 70 unidentified. This presupposes that out of the hundred we could say fairly definitely that 30 of those events are earthquakes. This leaves 70. In another place it says 30 percent of 70 would be 21. Twenty percent of 100 would be 20. So we are actually talking about 20 and 21 inspections. The important part of this is that for some 15 months, I believe, up to this time—15 or 16 months—we have been talking with the Soviets about a quota. They had continusouly said that we must accept a quota of inspections but we were never able to get them to say how many that quota represented. They would never define the word quota in terms of tangible

*See app. 2, p. 527.

numbers after 15 or 16 months of negotiations. Then our Government came forward and made a suggestion which for the first time set a tangible number of inspections. The communique of that day did not bring this to the attention of the public at all. It is important that the public know that the U.S. Government has been trying to solve this problem and have even gone so far as to make a proposal. I am not passing judgment on whether the proposal is a good one or not, but at least we did make a proposal.

As far as I know to this date the Soviets have shown no interest in it. They have not said they would accept it. They have not said that it is 10 times too large or that it is 5 times too large. They have just ignored the fact that we have offered a definite number. I think it is well to put in the record at this point from the official document. These documents will be part of the official record, all of them. (See app. 2, p. 527.)

Senator GORE. The doctor has just testified with respect to inspections whether it is 1, 20, or 100, "since no agreement was reached on the basic technical data, it was clearly impossible to reach agreement on the formulation of the criteria for on-site inspection despite the prominence of this item in our list of agenda."

Even though they agreed to 20 they still have not agreed to the kind of inspection or the criteria for the inspection, as you have told us.

Dr. PANOFSKY. If I could comment on this. I believe there is some misunderstanding here. What I was referring to here was the work of our Technical Working Group. In this Technical Working Group we did not agree on criteria. Then as the political negotiations progressed, we introduced the threshold concept, and the numbers which Mr. Holifield read from Mr. Wadsworth's statement are pretty much in agreement with my testimony on the technical subject. Now, the present proposal under discussion which was introduced here is to make a choice out of the number of unidentified events—out of the 70 which are being talked about—just on a free choice basis.

Representative HOLIFIELD. Random basis.

Dr. PANOFSKY. But to go from 100 to the 70, criteria would have to be applied, because if 100 are the total number of detected events, 70 would be the number of unidentified events. However, if no agreement is reached in the future on how to go from 100 to 70, this would still not be a very major point. So the main thing which this proposal has done is to make it less important whether we do or do not agree on criteria.

If we are allowed to make the choice of inspections we can apply our criteria. So it is not particularly necessary whether the method of going from 100 to 70 is agreed or is not agreed.

Senator GORE. Doctor, it is not we who will make the inspection; it would be an international control organization that would make the inspection according to the plan. This international organization would be subject to conduct by agreed criteria and procedure. Is that not true?

Dr. PANOFSKY. There are two steps. Whether the method to go from 100 to 70 is arrived at by an international organization, depends on the way the negotiations are at present—I have not followed the negotiations for the last weeks. The choice of which of the 70 to inspect would be by the other country. I think that is the present situation.

Senator GORE. Mr. Chairman, I would like to conclude by associating myself with your commendation of these distinguished gentlemen, Dr. Brown and Dr. Panofsky, for giving us this factual and objective information. They recognize, as all men must recognize, that there are considerations here other than scientific and engineering in character, and that the ultimate decision by the Government is partly a political decision. Decisions partake of two characterizations. One is information. This is what you gentlemen have given us, for which I am grateful. The second is judgment of that information. Upon information and judgment or from information and judgment can, I hope, flow a correct decision. I am perfectly willing to recognize that in addition to the risk involved and the scientific and engineering facts, there are moral questions involved. There are security questions involved. I wish to conclude by again expressing to you gentlemen my appreciation for your appearance.

Representative HOLIFIELD. Thank you very much, gentlemen, and Dr. Panofsky, you have been very helpful to the committee.

Dr. Brown, we will see you again on the panels.

(Dr. Panofsky's full statement on Technical Working Group II follows:)

A PRESENTATION OF THE RESULTS OF TECHNICAL WORKING GROUP II

I should like to report on the work of Technical Working Group II, which met in Geneva in November and December 1959 to consider the problem of detection of underground nuclear explosions. As you know, the American delegation to the Conference on the Discontinuance of Nuclear Weapons Tests tried to reopen the question of control of underground nuclear explosions several times during late 1958 and 1959. On January 5, 1959, the American delegation tabled a working paper which presented the reasons they considered the underground detection situation changed. The working paper (GEN/DNT/25) was based on new data obtained during the Hardtack Oct. 58 series. It maintained (1) that the number of earthquakes equivalent to a given yield explosion which would occur naturally through the world was larger than had been assumed by the experts in 1958, and (2) that identification of earthquakes was more difficult than the experts had assumed, particularly by the so-called first motion method. During the following months, the American delegation frequently pressed for a reexamination of the technical situation. Agreement for such reexamination was finally reached, and Technical Working Group II, with Dr. J. P. Fisk as Chairman of the U.S. technical delegation was convened on November 25, 1959.

The context in which this group worked was a very difficult one, much more so than in connection with Technical Working Group I on high-altitude detection. The Russian delegation were acquainted with our opinion that the capability of the Geneva system was less than had been believed, and they were concerned about our political objective in this working group. The Russian political leaders were presumably concerned with the possibility that as a result of the work of this group their delegation would be forced to sign a statement jointly with the United Kingdom and the United States that the situation concerning control was very much inferior to the situation believed to exist in 1958, and that as a consequence the chance of the negotiations' success would be impaired. Because of these worries, a substantial debate originated in the Conference concerning the terms of reference of the group which was never fully resolved. The United States wanted the terms of reference to include a reevaluation of the capability of the Geneva system based on new data; the Russians wanted to agree only to the presentation of the new data and of suggestions for the improvement of the system. The matter was finally compromised by having the terms of reference [attachment (1)] include the discussion of the new data, suggestions for improving the system, and consideration of criteria by which suspected seismic events could be helpful for onsite inspection. The question as to whether an evaluation of the system was to be included was bypassed, but it was the U.S. view that discussion of the new data and studies when entered into the report of the working group would in fact constitute such an evaluation.

The working group began with the presentation by the United States of new data from the Hardtack series. Some of the data had already been released to the Conference by the January 5, 1959, paper, but a large amount of further detail was provided, including a set of the original seismograms. It became clear immediately that it would be very difficult to obtain agreement on any matter which would make it appear that the experts' control system was less powerful than had been believed, but that the U.S.S.R. delegation would press continually to obtain agreement on the various potential improvements to the system. The U.S. delegation took the attitude that all facts, whether they made the system look better or worse, had to be considered. Progress under these circumstances was quite slow, particularly since many of the technical matters were very complex and hard to deal with in formal sessions; the Russian delegation would not agree until the last week of the meetings to our proposal for informal discussions among the scientists, although this had been very effective during the Conference of Experts and during Technical Working Group I.

There were extensive discussions on the problem of the number of earthquakes which would be expected worldwide to occur corresponding to a given yield nuclear explosion. This matter had been considered by the experts, and the numbers available at that time were used in arriving at their conclusions. The shots of the Hardtack series had indicated that the seismic signals produced were somewhat smaller than had been assumed by the experts; note that only one underground explosion had been fired prior to the 1958 Conference. In deriving information from the Hardtack data about the number of earthquakes expected corresponding to a given yield nuclear explosion, the matter is greatly beclouded by the inaccuracy of our seismic information concerning earthwide occurrence of small earthquakes. Specifically, a body of information on the frequency of occurrence of small earthquakes exists only for California and New Zealand, and going from this information to worldwide estimates, and particularly to estimates for the Soviet Union, involves many uncertainties; these uncertainties are in fact considerably larger than changes in the situation created by the new data of the Hardtack series. This issue is therefore not an important new consideration, and it now appears that probably its importance had been overemphasized in the United States. In discussing the matter with the Russians, however, we were faced with deliberate attempts by their delegation to force the estimates of the frequency of occurrence of earthquakes to a level well below the range of admitted uncertainties. The issues involved here are highly technical and have to do with such problems as tying together the seismic scales of magnitude between different regions of the earth, and the question of how to handle a scattered group of data in a statistically valid way. In the Russian presentations, attempts were made to reject data which made the estimated frequency of occurrence high while retaining data in the opposite direction. Also, the Russians introduced during the last sessions of the working group a new interpretation of the seismic magnitude scales which was totally unjustified, but which was repeated in their final report despite its rather obvious absurdity. The discussions of this issue went on throughout the sessions of the working group and were not resolved. I consider thls matter particularly unfortunate since the changes which are involved here according to our analysis are substantially smaller than the uncertainty with which the number of earthquakes equivalent to a given yield explosion can be determined without further research.

In the experts' report, the only method treated in some detail for distinguishing earthquakes from explosions is that of so-called first motion. When an explosion occurs, the earth initially moves away from the explosion in all directions and, therefore, the first motion observed by a seismometer should be a compression. When an earthquake occurs, a large mass of earth shifts relative to another mass as a stress in a fault is relieved; as a result, the seismic signal produced by an earthquake is essentially a compression in some directions while it is a decompression or rarefaction in others. In principle, if the initial seismic wave received by any one seismograph is in a direction to indicate a rarefaction, then the source cannot be an explosion. The situation is unhappily not this simple, since the earth's crust, even in the absence of earthquakes or explosions, is never completely at rest but undergoes some continuing movements as a result of the movement of the oceans, effects of wind, and other disturbances. Hence the signal from an earthquake or explosion is always superposed on such signals of seismic noise. If the first motion seen by a seismometer is submerged in the noise, one can confuse the backswing of the seismometer, or second motion, with the first motion and thereby confuse a rarefaction with a compression. The Hardtack series indicated that, with the seismometers used, the first motion was smaller in amplitude relative to the subsequent movements than had been estimated before,

and as a result the question of the noise hiding the correct answer is more serious than had been thought.

These data were presented to the group at Geneva, and were attacked almost immediately by the Soviet delegations on a purely formalistic basis—that the instruments used by the American seismologists did not meet the specifications for such instruments set down by the experts. The assumption in this criticism was that it would be true a priori that the instruments proposed by the experts would be better than those in fact used. The relevant differences in the instruments were as follows: (1) The experts recommended that groups of 10 seismometers be used rather than the single instruments used in almost all the American stations; (2) a seismometer has a so-called natural period, which is the period of free swing of the seismometer after it is disturbed. The experts specified that the natural period should be one second (although as it turns out there is some ambiguity in the wording of the experts' report in this respect) and the American instruments had a natural period of about one-third of a second. Any criticism based on the first of these differences is totally invalid, since the effect of using groups of 10 and more instruments rather than single ones had been measured by the American seismologists and found to be in general agreement with expectations. In the analysis of the data we had already corrected for the expected improvement of going from 1 to 10; thus there should have been no controversy on this point. The second criticism also turned out to be invalid—not because the instruments were not in fact different from those specified by the experts (this is a matter of interpretation of the experts' report), but because we were able to show conclusively that the instruments used were actually better than those recommended by the experts in regard to the problem of distinguishing the direction of first motion in the presence of seismic noise.

The correctness of this technical fact was demonstrated during the meetings in two ways: First, by mathematical analyses carried out during the meetings; and second, by carrying out on very short notice an experiment in Oklahoma in which seismometers of the two characteristics to be compared were prepared, and very fortunately produced a clear record of an earthquake which occurred on December 9, 1959, during the time of the meetings and which showed the superiority of the instruments used during the Hardtack series in distinguishing the direction of first motion. Despite the fact that the technical situation had thus been clarified, the Russian delegation maintained their position by retaining in their final report the earlier criticism of our results as unworthy of consideration since the "correct" instruments were not used.

It is indicative of the problems encountered during these meetings that it was not initially clear within the U.S. delegation itself when the Soviet delegation raised these objections whether they were in fact valid. It was only the analysis carried out during the sessions and the successful experiment so rapidly carried out that demonstrated the validity of the American instrument readings. I note here that in their final report the United Kingdom delegation stated their complete agreement with the American technical points.

The Russians also criticized the U.S. delegation on the ground that our "new data" presented during the working group did not differ significantly from the material introduced to the Conference in January 1959. This criticism is to some extent valid since, as the example given above indicates, not a great deal of further analysis of the data was carried out in 1959.

The next new information introduced concerned the problem of decoupling, which is possible by detonating an underground explosion in a large hole. At the time of the meetings of the working group there were only preliminary experimental data on this point from work carried out by the British and presented by their delegation. Our estimates consisted of a combination of theoretical calculations and measurements made during the earlier Rainier explosion, and this made a very convincing story that seismic signals could be reduced by about a factor of 300 relative to the conditions pertaining at the Nevada test explosions. These calculations and data were introduced by our delegation, and were first attacked on technical grounds by the Soviet group. After detailed discussion indicated that the U.S. calculations very likely were correct, the Russians changed their position from technical attacks on our conclusions to the statement that they were "premature."

Material was introduced by both the United States and Soviet delegations on many suggestions for improvement of the experts' system and for improved methods of analysis of seismic signals which could be recorded. There was very little disagreement on these; after discussion of the methods, the results were agreed upon by all delegations.

The technical situation here is very unfortunate. There exist a large number of potentially useful sources of seismic information which have bearing on the problem of distinguishing earthquakes from explosions. They are all based either on theory—which, in view of the complexity of the earth's crust, might not be in accordance with observations—or on very incomplete experimental observations. Among such methods are the use of long-period waves and their interpretation, reduction of noise by burying seismographs in deep holes, and improvement of the operating characteristics of seismic equipment in general. I shall not discuss these in detail since they will be dealt with in further testimony during these hearings. The principal problem is that these methods are potentially useful, but within the present state of knowledge there is no way of evaluating quantitatively how useful they could be nor is it possible to write specific recommendations under which they could be used by unskilled operators. In the hands of skilled seismologists, even at present the use of some of these methods might very well improve the judgment which can be exercised in distinguishing earthquakes from explosions.

Many of the improvements possible had been discussed previously and are described in the publicly released Berkner report. In addition to the possible methods introduced at the Technical Working Group, the Berkner Panel also considered the possibility of introducing unmanned stations as "slaves" of the control posts. These stations would probably improve the system performance significantly, but at the same time add a great deal to the cost and complexity, in particular in regard to the communication networks required. In addition, such stations would require more personnel at the control posts and access of these personnel to the unmanned stations. None of these factors had been evaluated with sufficient care prior to the work of Technical Working Group II to enable us to discuss them in the negotiations.

The next major topic discussed was the question of "criteria for initiating onsite inspection." Whenever a seismic event is recorded on the instruments of the control system, the decision must be made whether it is eligible for onsite inspection. The system designed by the experts has enough sensitivity that it would detect but not identify roughly 15,000 earthquakes a year on a worldwide basis. Note again that this estimate is uncertain by a factor of about two or possibly more. Although all parties have agreed in principle that a certain number selected from these events should be eligible, neither the mechanism for making the selection nor the number of events allowed to be inspected has been agreed upon. It is clearly desirable first to reduce the number of eligible events by using the seismic information produced by the control system itself.

Depending on the accuracy to which the instruments of the control posts have been calibrated, a detected event could be located in an area between 50 and 300 square miles. Eligibility for inspection is presumably confined to such an area. If a certain number of detected events can be definitely identified as earthquakes, they would no longer be eligible for inspection. We have called criteria which definitely identify a seismic event as an earthquake "negative" criteria. If a seismic event can be identified with a high degree of confidence as an explosion, then it could in principle be made mandatory that an onsite inspection be made. I shall call such criteria "positive." The U.S. delegation formulated a rather complex set of negative criteria which would identify a certain number of seismic events as earthquakes but would leave a large fraction of the remaining events eligible for inspection. In arriving at these criteria, the U.S. delegation assumed that these criteria must not be so stringent that in case a nuclear explosion did occur more than half of such explosions would not lose their eligibility for inspection by virtue of these criteria. Based on the knowledge available to the delegation, these criteria turned out not to be very powerful, and in particular almost all seismic events of small magnitudes would remain eligible. The U.S. delegation was unable to formulate any positive criteria, beyond agreeing with the other delegations that seismic events originating in those areas which are usually considered "aseismic" (in which earthquakes, to our knowledge, do not occur) should be subject to inspection. The Soviet delegation took the position that it was the duty of the working group to formulate criteria which would remove eligibility for inspection from all but a very small fraction of seismic events. The fact that the question of whether this can be done is a technical rather than a political one did not change their position. In several interesting exchanges, the Russians insisted that if science was required to find solutions to a problem, then such a solution would necessarily be found. The Soviet delegation introduced a set of criteria which would in fact remove eligibility for inspection from a very large number of seismic events, including the explosions

of the Hardtack series [!]. The Russians did not include these in their final report, and we believe that they were not intended to be taken as a serious proposal.

The difficult question which faced us during the discussions and which faces us now is how to make use of the large body of seismic information called auxiliary or diagnostic aids, which might be useful but which cannot now be codified into specific criteria and whose effectiveness we cannot now evaluate. If choice of events to be inspected is left to one of the parties carrying out the inspection on the territory of another, then this auxiliary information might be helpful to an as yet unknown extent. At the same time, as knowledge progresses, some of this information might grow into useful formal criteria. Beyond acknowledging this state of affairs, however, there was no way in which the U.S. delegation could responsibly introduce the use of these aids into the discussion.

Since no agreement was reached on the basic technical data, it was clearly impossible to reach agreement on the formulation of the criteria for onsite inspection, despite the prominence of this item in our list of agenda.

With the exception of the suggested improvements to the detection system, the working group ended with a disagreed report—really a letter of transmittal with three reports appended.

I should like to comment briefly on the relation of the results of Technical Working Group II to the American "threshold proposal" to the Conference and to the recent Russian proposal to accept the threshold in combination with a moratorium on underground nuclear tests. The U.S. proposal attempts to relate the proposed test ban to the capability of the system for detection and identification. This is done by defining a threshold in terms of a seismic magnitude such that only tests giving rise to signals above such a magnitude would be banned. The magnitude of 4.75 was chosen, which corresponds to the seismic signal produced by about a 20-kiloton nuclear explosion, closely tamped, and fired under the conditions of the Nevada test site. At this value the total number of seismic events remaining eligible for inspection would be small, so that it is believed that 20–30 actual inspections a year in the Soviet Union might be both acceptable to the U.S.S.R. and satisfy our requirements for adequate control. At the same time, the threshold is set at such a high level that the incentive for carrying out large-hole decoupling would be reduced since holes which would provide decoupling for explosions much larger than 20 kilotons might have to be very large.

The principal concession made by the Soviet Union in accepting the threshold concept is the implied acknowledgment that there do exist substantial problems in the detection and identification of underground explosions; their acceptance is indicative of their willingness to negotiate on our technical terms. Despite the disagreement in Technical Working Group II, its work did serve the purpose of conveying, apparently convincingly, the technical problems involved in detection of underground explosions, and did make it possible to carry out further negotiations on a political rather than a technical basis.

[Attachment No. 1]

CHRONOLOGY

November 24, 1959: Terms of reference adopted by Conference on the Discontinuance of Nuclear Weapon Tests.

November 25, 1959: Technical Working Group II met for first of 21 formal meetings.

November 26, 1959: Agenda adopted (GEN/DNT/TWG.2/1).

December 18, 1959: Technical Working Group II adjourned.

December 19, 1959: Report of Technical Working Group submitted to the Conference.

TERMS OF REFERENCE

"The Technical Working Group of experts shall consider the question of the use of objective instrument readings in connection with the selection of an event which cannot be identified by the international control organ and which could be suspected of being a nuclear explosion, in order to determine a basis for initiating on-site inspections. As part of their work, the experts, proceeding from the discussions and the conclusions of the Geneva Conference of Experts, shall consider all data and studies relevant to the detection and identification of seismic events and shall consider possible improvements of the techniques and instrumentation."

AGENDA

"1. General statements.

"2. Consideration, proceeding from the discussions and the conclusions of the Geneva Conference of Experts, of all data and studies relevant to the detection and identification of seismic events and of possible improvements of the techniques and instrumentation.

"3. Consideration of the question of the use of objective instrument readings in connection with the selection of an event which cannot be identified by the international control organ and which could be suspected of being a nuclear explosion, in order to determine a basis for initiating on-site inspections.

4. Preparation of a report containing conclusions on matters discussed above."

Representative HOLIFIELD. Our next witnesses are Dr. Carl Romney of the Air Force Technical Applications Center, and Dr. Richard Latter of the Rand Corp. Dr. Romney was a member of the U.S. delegation to the Geneva Conference and also a member of Technical Working Group II. Dr. Latter was a member of the U.S. delegation at the Conference of Experts. He was also a member of Technical Working Groups I and II. These gentlemen will testify on means for improving seismic detection.

STATEMENTS OF DR. CARL ROMNEY,[1] AIR FORCE TECHNICAL APPLICATIONS CENTER, AND DR. RICHARD LATTER,[2] THE RAND CORP.

Dr. ROMNEY. We were asked to discuss first the numbers of unidentified events, and in the interest of time I would like to request that my written statement which describes the mechanism by which we get our estimates of the number of earthquakes simply be incorporated into the record.

Representative HOLIFIELD. Dr. Romney, I believe your profession is that of seismology.

Dr. ROMNEY. Yes, sir.

Representative HOLIFIELD. Your biography, which will be in the record at this time, says that you were a meteorologist with the U.S. Navy from 1945 to 1946. You were research assistant in seismology at the University of California, 1947 to 1949. Seismologist, later chief seismologist, later consulting seismologist from 1949 to 1955. Geophysicist and later assistant technical director of the Air Force Technical Applications Center. I have already indicated that you were a member of the U.S. delegation to the Conference of Experts.

Dr. ROMNEY. Thank you.

Representative BATES. Mr. Chairman, if I may.

Doctor, in the field of your own competency, do you agree with the statements that have been made here today?

Dr. ROMNEY. Some of them.

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[1] Born: Salt Lake City, Utah; June 5, 1924.

Education: B.S. (meteorology), California Institute of Technology, 1945; Ph. D. (geophysics), University of California, 1956.

Employment: 1. Meteorologist; U.S. Navy; 1945–46; 2. research assistant in seismology, University of California, 1947–49; 3. seismologist, later chief seismologist; Beers & Heroy, consulting geologists, geophysicists, and engineers; 1949–55; 4. geophysicist; later assistant technical director; Air Force Technical Applications Center; 1955– .

Related professional experience: 1. Member of U.S. delegation to Geneva Conference of Experts, 1958; 2. member of Panel on Seismic Improvement (Berkner Panel), 1959; 3. member of U.S. delegation to Conference on Discontinuance of Nuclear Weapons Testing, Technical Working Group 2, 1959.

[2] Received a Ph. D. in theoretical physics at the California Institute of Technology in 1949. From 1949 to 1957 he was a staff physicist at the Rand Corp. Since 1957, he has been the head of the physics division of the Rand Corp. Dr. Latter was an adviser to the U.S. delegation of the Conference of Experts 1958, a member of the U.S. delegation of the Conference on the Discontinuance of Nuclear Weapons Tests, and a member of the U.S. delegations of Technical Working Groups 1 and 2. Dr. Latter has recently been appointed chairman of the ad hoc group on seismic and high altitude detection to the Department of Defense.

Representative HOLIFIELD. I will guarantee you that the witnesses that will be called before this committee will not agree 100 percent.

Representative BATES. I just wanted to bring that up. Will the doctor be on the panel later on?

Representative HOLIFIELD. Yes, he will.

Your statement will appear in full in the record.

(Dr. Romney's prepared statement follows:)

NUMBERS OF SEISMIC EVENTS AS A FUNCTION OF SIGNAL STRENGTH

INTRODUCTION

The statistics on the annual numbers of earthquakes which occur throughout the world are compiled in terms of an earthquake "magnitude" scale. As an illustration, it has been found that during an average year about 170 shallow focus earthquakes of magnitude greater than 6 occur, and it has been estimated that about 1,400 shallow focus earthquakes of magnitude greater than 5 occur. Of more relevance to the problems of monitoring a nuclear weapons test moratorium, however, are the numbers of seismic events of magnitude equal to or larger than the seismic disturbance caused by an underground nuclear explosion of some given yield. These numbers may be obtained if the equivalent earthquake magnitudes of underground nuclear explosions are known. However, such estimates of numbers of earthquakes equivalent in size to underground explosions are very imprecise, for reasons discussed below.

EARTHQUAKE STATISTICS

An underground nuclear explosion of, say, 20 kilotons corresponds in seismic effects to a very small earthquake—one so small that it could go virtually unrecorded at the existing seismographic stations in many parts of the world. Our knowledge of the total numbers of such small earthquakes is far from precise. Indeed, adequate statistics exist in only a few selected parts of the earth where there are dense networks of seismographic stations, particularly in southern California and New Zealand. For most other parts of the earth, good statistics exist only for very large earthquakes. Estimates of the numbers of small earthquakes for most of the world are therefore obtained by assuming that the numbers increase with decreasing magnitude in the same proportion as the earthquakes of California and New Zealand. For this reason alone, the statistics for most parts of the world may well be uncertain by a factor as large as 2, i.e., if the best estimate for the number at a given magnitude is 1,000 annually, we should be quite prepared to find as many as 2,000 or as few as 500. In addition, there are relatively large fluctuations in the number of earthquakes occurring within a given area from year to year.

About half of the world's earthquakes can be expected to occur on the continents, or in coastal and inland sea areas where hydroacoustic signals could not be expected to propagate to great distances, and therefore where only the seismic technique could be used for explosion detection purposes. About one in six of these earthquakes can be expected to occur in the U.S.S.R. and China combined; or about 1 in 10 can be expected to occur in the U.S.S.R. alone. Similarly, about 1 in 10 can be expected to occur in the United States, mostly in Alaska and the Aleutians. Estimates of the numbers of earthquakes equal to or larger than a given magnitude are shown in figure 1. It should be remembered that the indicated numbers may be either overestimated or underestimated by a factor of about two.

MAGNITUDE EQUIVALENCE OF EXPLOSIONS

The Hardtack explosions of October 1958, and the Rainier explosion of 1957 provide us with data on the signal sizes to be expected from explosions in one particular material; namely, Oak Springs tuff. These data showed that a 19-kiloton explosion produced signals equivalent in size to a small earthquake of magnitude of 4.75; a 5-kiloton explosion produced signals equivalent in size to an earthquake of magnitude 4.4; and a 1.7-kiloton explosion produced signals equivalent in size to an earthquake of magnitude 4.06. Using statistics compiled on large earthquakes throughout the world, we are able to estimate that there will be between 1,000 and 4,000 earthquakes annually (with the best estimate

being 2,000) equivalent in size to a 20-kiloton explosion in tuff. It is similarly estimated that there will be between 2,500 and 10,000 earthquakes annually (with the best estimate being 5,000) equivalent in size to a 5-kiloton explosion in tuff, and between 7,500 and 30,000 earthquakes annually (with the best estimate being 15,000) equivalent in size to a 1-kiloton explosion in tuff. Figure 2 shows the estimated numbers for various equivalent yields between 0.5 and 100 kilotons. These numbers are in agreement with the estimates by the U.S. delegation at the Conference on the Discontinuance of Nuclear Weapons Tests, Technical Working Group 2.

It is also known that the material in which an explosion occurs can affect the size of the seismic signals generated. Since the total numbers of earthquakes vary with magnitude this, in turn, means that the number of earthquakes equivalent in size to a given nuclear explosion will depend in part upon where the explosion takes place. Our knowledge of the size of this effect is very limited. Thus, the uncertainties associated with coupling conditions at the source, combined with the uncertainties in the numbers of earthquakes, make the estimates of figure 2 very approximate indeed.

ESTIMATES OF UNIDENTIFIED SEISMIC EVENTS

Dr. Richard Latter will present the most recent estimates of the numbers of seismic events which will remain unidentified after analysis of data from the control system proposed by the Geneva Conference of Experts. Earlier estimates are tabulated below for completeness.

(The data referred to follow:)

TABLE 1.—*Estimated annual number of unidentified continental earthquakes (worldwide)*

[Yield]

Source of estimate	5 kilotons [1] and greater	10 kilotons [1] and greater	20 kilotons [1] and greater
Conference of Experts	20–100		
Berkner Panel, experts network and equipment	[2] 1,500	[2] 400	[2] 60
Berkner Panel, experts network, improved equipment	[2] 300	[2] 40	[2] 15

[1] Shots in tuff.
[2] Numbers could be half as large or twice as large, i.e., "300" means "between 150 and 600."

FIGURE 1

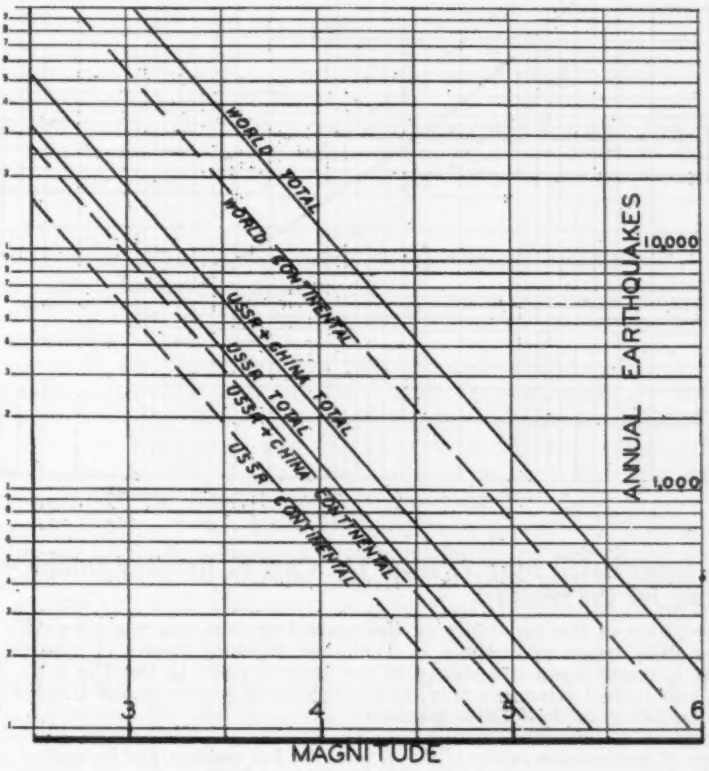
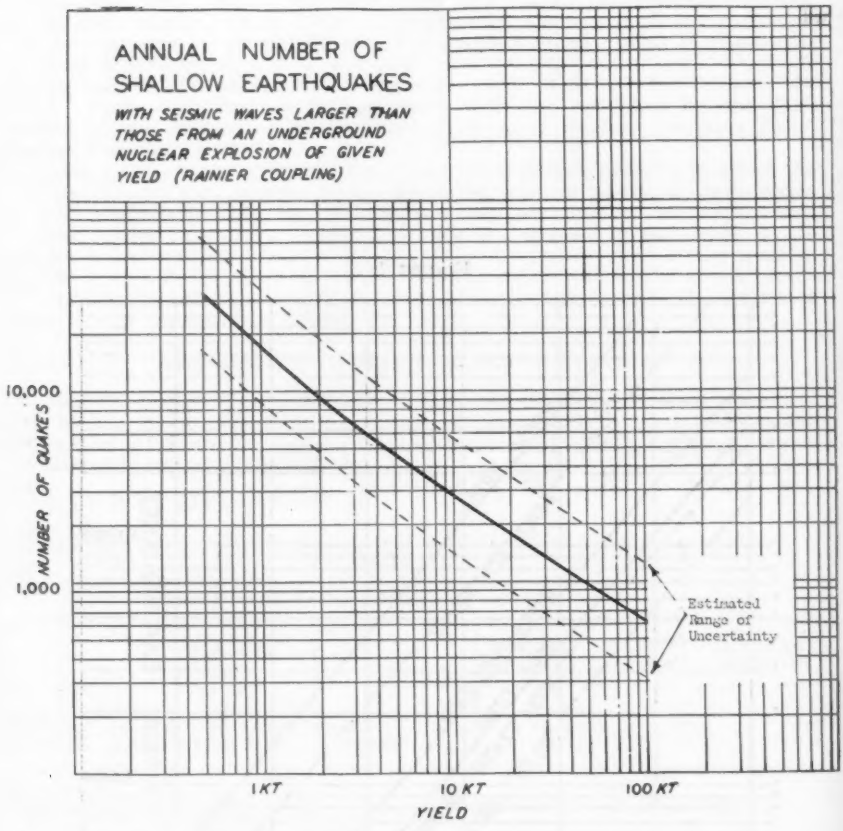

WORLD TOTAL
WORLD CONTINENTAL
USSR + CHINA TOTAL
USSR TOTAL
USSR + CHINA CONTINENTAL
USSR CONTINENTAL
ANNUAL EARTHQUAKES
10,000
1,000
3
4
5
6
MAGNITUDE

FIGURE 2



(By letter dated May 7, 1960, Dr. Carl F. Romney submitted the following for the record:)

An estimate of the capability of the control system was made by AFTAC in January 1960, based upon the U.S. Technical Working Group II criteria. This estimate is based upon the ability of the control posts in the U.S.S.R., United States, and United Kingdom only, to identify earthquakes in the U.S.S.R. The results are found in the following table:

Numbers of earthquakes in the U.S.S.R. detected but unidentified by control posts in the U.S.S.R., U.S., and U.K.

Number of earthquakes	Magnitude	Equivalent yield (kilotons)	Number of unidentified earthquakes	
			100-detector array	10-detector array
620	4.0	2	560	590
320	4.3	4	260	290
170	4.6	11	110	140
123	4.75	19	70	100
72	5.0	40	35	45
47	5.2	80	20	24

Dr. ROMNEY. Could I just say in summary of my prepared statement on the numbers of events that the main points contained in my prepared statement are, first, that the numbers are very imprecise, because we don't have good information on numbers of small earthquakes all over the world and, second, because we don't have good information on the size of the seismic waves produced by nuclear explosions in material other than tuff.

Now I would like to ask Dr. Richard Latter to give the most recent and probably the best estimates which have been made of the numbers of unidentified events which can be expected to result from the operations of the control system.

Dr. LATTER. My remarks will concern the capabilities and limitations of a control system to detect and identify seismic events. To put what I have to say in proper perspective, I would like first to make clear two vital points. The first is—we know that the seismic signals from an underground nuclear explosion can by deliberate effort be reduced by a factor of 300 or more.

Representative HOLIFIELD. Let us understand what that means. Does that mean that a 300-kiloton shot could be reduced in seismic recordings to a 1-kiloton recording?

Dr. LATTER. Yes, sir.

Senator GORE. By what process?

Dr. LATTER. This is by means of the large underground cavity, the so-called decoupling process.

Senator GORE. Do you agree with that?

Dr. ROMNEY. Yes, indeed.

Senator GORE. Are the other two scientists still here?

Representative HOLIFIELD. To clarify the record, I think Dr. Panofsky and Dr. Brown testified on the recording of a 20-kiloton shot under the conditions of the Rainier shot, which was not a decoupled shot and which was in tuff type of earth in Nevada, and they did not go into the decoupling. Is that right?

Senator GORE. Yes. I asked specifically about a 50-kiloton under decoupling and Dr. Panofsky said the chance of detection would be, I believe, one time negligible and another time zero. I didn't go so high as 300 and now you are telling us that the chance of detecting a 300-kiloton explosion would probably be nil.

Dr. LATTER. Yes, sir.

Senator GORE. And you agree with it?

Dr. LATTER. Excuse me. There is one question here regarding the feasibility of building a cavity of the size necessary to produce the decoupling factor of 300. This, I am sure, will be talked about in one of the later statements.

Senator GORE. In other words, assuming that a country is willing and able either to find a cavern or cavity beneath the earth or to dig one, that they could conduct a 300-kiloton test explosion and reduce it to the seismic signal of a 1-kiloton under conditions of the Rainier shot.

Dr. LATTER. Yes, sir.

Senator GORE. Thank you.

Representative HOLIFIELD. You may proceed.

Dr. LATTER. With the Geneva system, or any plausible modification of that system, even very large explosions might therefore go

undetected. Against these explosions the control system has no capability whatsoever and cannot provide a technical safeguard.

The second point I want to make is that the seismic signals from underground nuclear explosions are believed to be similar to the signals from some earthquakes. On this basis it is assumed that nuclear explosions cannot be identified by their seismic signals alone. A control system can only identify earthquakes. Inspection of the site of an unidentified event, and therefore a suspected event, is required for the purpose of obtaining positive proof of the cause of the event. To prove that a nuclear explosion has taken place it is necessary to obtain a sample of radioactive debris.

Keeping these limitations in mind at all times, we would like to be able to estimate the capability of a control system to detect and identify seismic events in the Soviet Union and in the United States. To make such an estimate, however, it is necessary to have a great deal of information. The detailed location of the control stations must be specified. The frequency and distribution of earthquakes must be known. The seismic noise conditions of the control stations must be known—noise interferes with the detection of seismic signals and limits the capability of a control system. Finally, it is necessary to specify a criterion for identifying the signal from a given seismic event as being natural in origin, that is, earthquakes. Available data on all these aspects of the problem are meager and inconclusive.

Some of the uncertainties can be eliminated by a continuing research program in seismology, directed at those questions pertinent to the detection problem. Some, however, can only be eliminated through free access to the Soviet Union—specifically, the availability of suitable quiet sites for control stations, and the determination of the number of earthquakes occurring within the Soviet Union, particularly the low magnitude earthquakes.

Senator PASTORE. May I ask a question at this point, Mr. Chairman?

Representative HOLIFIELD. Senator Pastore.

Senator PASTORE. I ask this question in the concept of this idea of whether or not a voluntary moratorium of underground shots below the recommendation of 4.75 should be for 1 year, 2 years, 4 years, or 5 years. Let us assume that earnestly both sides entered into an agreement for research in the field about which you are talking now and neither side would suspect the other side of dishonesty or insincerity in their drive to reach a technical answer with regard to this detection that we are talking about. In your opinion, how long would it take to discover this control system that we want to achieve?

Dr. LATTER. Sir, on a purely scientific basis there is no assurance whatsoever that you will find in any length of time either improvements in the system or improvements in concealment against the system. You simply cannot predict the course that the research will take.

Senator PASTORE. The reason I ask the question is because there has been an intimation that the Russians were asking for a moratorium of 5 years. There has been some reluctance on the part of the free countries to agree. They think it should not be more than 1 year. In the context of that, the argument has been made that if either side is sincere and earnest in its cooperation with regard to developing this joint venture of research the agreement might be continued from

time to time. The question I am trying to formulate is this—I know it is speculative—in your opinion would it be that this moratorium of 1 year might have to be continued five or six times before you would find the answers even if both sides were earnestly working at the research that might be necessary to discover the adequate system?

Am I correct in that speculation?

Dr. LATTER. I would say it is certainly true that it will take time to learn the answers that we are looking for to improve detection. It will take time to learn about improved techniques in concealment. You will learn more and more with each year that goes by. Whether you learn the answers that you need to decide on an adequate control system in a year or 2 years or 5 or more, I simply do not know.

Senator PASTORE. Let me ask you this further question: Leaving out the political considerations which are necessarily intertwined in this whole process, but considering alone the technical aspects involved, is it your considered opinion that even though both sides were in earnest in this research that there would be a considerable amount of calculated risk involved even on a voluntary system that was negotiated for 1 year and might have to be continued from time to time. Do you understand my question?

Dr. LATTER. Not entirely, sir.

Senator PASTORE. Let me repeat it again. Leaving out the political aspects involved, and considering now only the technical aspects, assuming that both sides were in earnest in developing this research which is necessary, and they negotiated a voluntary moratorium that would be declared by the heads of government on both sides, of which we have heard so much in this proposed plan, and let us assume that the President's suggestion were accepted of a shorter term rather than a long term being suggested by Mr. Khrushchev—which I think is 5 years—is it your considered opinion that no matter what choice you took there is a considerable calculated risk involved?

Dr. LATTER. Calculated risk with respect to detection?

Senator PASTORE. Yes.

Dr. LATTER. If you are not allowed to inspect below magnitude 4.75——

Senator PASTORE. Which would not be contemplated under a moratorium.

Dr. LATTER. Then I would say that all explosions that produce a signal below magnitude 4.75 would go unidentified as explosions by any control system.

Representative HOLIFIELD. Proceed.

Dr. LATTER. I was speaking, just before the question, about the uncertainties in determining the capability of the control system. Some uncertainties, and perhaps the most important ones, concern the nature of the seismic signals from unconcealed underground nuclear explosions. So far only six such underground nuclear explosions have been undertaken by the United States. These experiments have several possibly vital limitations. They were all conducted in the same environment at essentially the same depth below the surface and in essentially the same geological conditions. These limitations are important because the amplitude of the first ground motion, which presently is the only basis for identifying underground seismic events as earthquakes, may depend upon these parameters. They are also important in connection with all the diagnostic aids that have been

suggested for discriminating earthquakes from nuclear explosions. We do not know at present whether the Nevada explosion signals are characteristic of unconcealed underground explosions or whether they are a very special type of signal which will be different for explosions under other conditions. This uncertainty could be of overriding importance in evaluating the capability of a control system. For in using the characteristics of the Nevada signals as a basis to discriminate earthquakes from nuclear explosions, the possibility exists that all our conclusions will be proved wrong at some later time. Nevertheless, for the present that is all we can do and that is what we have done.

Specifically, we have taken the criterion given in the U.S. report of Technical Working Group II, which identifies a seismic event as an earthquake, provided the peak seismic signal is 20 to 40 times the noise background, and provide first motion rarefactions are observed at four stations. This criterion was believed by the U.S. Working Group II to be a good criterion, consistent with the Nevada underground nuclear explosion data. A good criterion should identify as many earthquakes as possible but should not misidentify nuclear explosions as earthquakes. When it came to other uncertainties such as the frequency of earthquakes in the Soviet Union all we could do was to make a good guess.

Before presenting the results for the system capability, however, I would like to remind you that the current U.S. proposal to the Conference on the Discontinuance of Nuclear Weapons Tests is for a threshold at magnitude of 4.75 which corresponds roughly to the 19 kiloton Blanca explosion. Therefore, our analysis is aimed primarily at the question: How many earthquakes per year of magnitude greater than 4.75 fail to be identified as earthquakes by the control system and could therefore be suspected of being nuclear explosions.

In our first study we assumed that control stations of the Geneva system were located in three countries only—14 stations in the United States, 1 in the United Kingdom, and 21 in the Soviet Union. While there are no agreed numbers of stations in the three countries, these assumed numbers are consistent with the agreed system requirements of the 1958 experts report. All of these stations were assumed to be equipped with the improved seismic instrumentation recommended by Technical Working Group II.

Representative HOLIFIELD. And that seismic instrumentation is available now. What I mean by that, it has been invented and designed and constructed in component parts and it could be put together on the basis of existing seismic instrument capability.

Dr. LATTER. That is my understanding, that the component parts for the detection system are the Benioff seismographs which have been known and are used. The question of putting them together in an array of 100 seismographs I don't know the answer to. I believe that has not yet been explored experimentally and this is one of the problems that is facing the research program.

Representative HOLIFIELD. But at least we are talking about a possible arrangement this time.

Dr. LATTER. Yes, sir.

Representative HOLIFIELD. And one which would not require a long period of research and development.

The instruments themselves. I am not talking about the history of their recordings.

Dr. LATTER. Yes, sir; that is correct. There are other types of equipment recommended by Technical Working Group II, long period instruments. I think, too, these are in existence. I am told by Dr. Romney that not all of the equipment that is recommended in Technical Working Group II does exist.

Representative HOLIFIELD. Some of it has to be developed.

Dr. LATTER. Yes, sir. But the important parts for identification are presently in existence.

To continue, the results of the study are—with this kind of control system there would be 173 earthquakes above the threshold per years in the United States which are unidentified and 53 per year in the Soviet Union. If the Geneva system of control stations is installed throughout the whole world, the number of such unidentified earthquakes in the United States is reduced to about 143 per year and in the Soviet Union to 28.

If we make the assumption that, say 30 percent of these unidentified events will be inspected, there will be 52 inspections per year within the United States in the first case, and 43 per year in the second—and 16 inspections per year within the Soviet Union in the first case and 8 per year in the second.

To make sure of these results we went back over our analysis step by step to see what are the critical assumptions on which the results most sensitively depend. We found that the criterion for identifying earthquakes was far and away the most important assumption. For this reason we decided to reexamine all the Hardtack seismic data, to be as sure as possible that the criterion used—the criterion of Technical Working Group II—was well founded.

The result of this study was that—we were not able to convince ourselves that the criterion of Technical Working Group II is a valid criterion. In particular, this criterion depends almost entirely on the capability of the control system to detect first motion from nuclear explosions at the great distance of 2,500 to 3,500 kilometers—the so-called far zone. Unfortunately, the Hardtack seismic data do not provide reliable confirmation of this capability.

The Hardtack data do show, however, that the first motion can be reliably detected within the so-called near zone, out to about 1,100 kilometers from the source.

Representative HOLIFIELD. How many miles is that?

Dr. LATTER. About 680 miles.

Also we found that at distances less than 700 kilometers, the first motion could be easily read if the peak signal is 10 times the noise, and at distances between 700 and 1,100 kilometers, if 20 times the noise, instead of the 20 and 40 used in the criterion of Technical Working Group II. On this basis we formulated a new criterion, one which depended only on the more reliable seismic data from the near zone. Because of the greater reliability we could also substitute for the requirement of four rarefactions, the less stringent requirement of only two rarefactions.

With this new criterion, which we believe to be well founded, we reevaluated the capability of the Geneva system. The surprising result is that the capability is substantially the same, and thus our criterion was as effective as that of Technical Working Group II, at the same time is well supported experimentally.

Having found a good criterion, we asked the question whether there were some minor changes that could be made to improve the Geneva system. Specifically, we asked the question whether the system could be substantially improved by increasing the number of control stations only slightly.

Our idea here was based on the fact that in the Soviet Union about 90 percent of all earthquakes occur in a small fraction of the total area of the Soviet Union, namely, about 5 percent.

Senator GORE. Is this area adjoining Red China? Is it in the mountainous area, the southern part of the Soviet Union?

Dr. LATTER. Yes, sir. One area which has about 30 percent of the earthquakes in the Soviet Union is on the border between China and the Soviet Union, as I recall.

Senator GORE. It seems to me that this raises the question of the possibility of detecting an underground explosion conducted beneath the Himalayas some few hundred miles in Red China. If this is the area where most of the earthquakes in Russia occur, is it not also the area in China where most earthquakes occur?

Dr. LATTER. No, sir. The Chinese earthquakes occur mainly in the central region of China, as I recall. The Pamir region is principally in the Soviet Union.

Senator GORE. If there were no stations at all in Red China, assuming that there was sufficient cooperation between Red China and Red Russia, that Russia would be permitted to conduct an experiment if she desired to do so, and if she did desire to do so within Red China what opportunity would there be of detecting an explosion of any magnitude whatsoever?

Dr. LATTER. Detecting an explosion?

Senator GORE. Identifying.

Dr. LATTER. The system has no ability to identify an explosion. It has only an ability to identify earthquakes. If you have no stations in China I simply do not know the answer to the question how many Chinese earthquakes would be identified by the system as earthquakes. I am sure it is quite small.

Senator GORE. Thank you.

Dr. LATTER. It seemed plausible that the addition of a few more stations in these seismic areas would make a big improvement.

Senator GORE. Would you not also say that just as the installation of additional seismic stations within this area would provide great improvement, the installation of stations on a worldwide system such as suggested by the Conference of Experts would likewise be a very great improvement?

Dr. LATTER. For events within the Soviet Union, are you speaking of those?

Senator GORE. I am speaking of those in one instance and to the conduct of tests in other countries, on the other.

Dr. LATTER. Certainly for the identification of the earthquakes outside the Soviet Union it will be quite essential to have stations outside the Soviet Union as well. If you are talking about identifying earthquakes within the Soviet Union only, then stations within the Soviet Union sufficient in number can give a very substantial capability. For the additions outside the system I can indicate in a little while what the added capability would be when you have the worldwide system. I think I did earlier, as a matter of fact.

Senator GORE. You did earlier. I have forgotten the figures. You did give that but that was only an indication of the number of stations in the Soviet Union recommended by the Conference of Experts.

Dr. LATTER. No, sir. I gave two cases. One case in which there were stations in only 3 countries, in which there were 53 events per year in the Soviet Union which were unidentified. I also gave the case of a worldwide system in which case there were 28. The equipment I was talking about was the improved equipment of the Technical Working Group II.

Senator GORE. That is where I misunderstood.

Dr. LATTER. It is the 100 seismograph array.

Senator GORE. But even in the case of Working Group II the installation of stations in countries other than the Soviet Union would reduce the unidentified seismic occurrences by some 50 percent.

Dr. LATTER. That is correct.

Senator GORE. Thank you.

Dr. LATTER. Accordingly, we reevaluated the system increasing the number of stations inside the Soviet Union from 21 to 25, not a very big increase. The result of this increase, however, was quite remarkable. Instead of about 50 unidentified events per year, there were now only about 20. Instead of 15 inspections, there were only 6. This is a big improvement for a small cost.

At this point it is natural to ask: How far can one go in this direction? And so we added more stations within the Soviet Union and reevaluated the system each time. Of course, the more stations added the better the system became. However, the rapid gains stopped after we added another 5 stations, making a total of 30. After that we had to add many stations to make small improvements. For the 30-station system we found that 9 earthquakes per year were unidentified above magnitude 4.75 and 36 per year above magnitude 4.35—magnitude 4.35 is equivalent to the 5-kiloton Logan explosion. With 30 stations in the Soviet Union—which is an increase of 9 stations over the assumed Geneva system—we obtained the important result that the capability of the system for identifying earthquakes within the Soviet Union was essentially restored to the capability estimated by the 1958 Conference of Experts.

Representative HOLIFIELD. Thank you very much for your statement.

(Dr. Latter's prepared statement follows:)

CAPABILITIES AND LIMITATIONS OF A GENEVA TYPE CONTROL SYSTEM

Richard Latter, the Rand Corp.

My remarks will concern the capabilities and limitations of a control system to detect and identify seismic events. To put what I have to say in proper perspective, I would like first to make clear two vital points. The first is—we know that the seismic signals from an underground nuclear explosion can, by deliberate effort, be reduced by a factor of 300 or more. With the Geneva system or any plausible modification of that system even very large explosions might therefore go undetected. Against these explosions the control system has no capability whatsoever and cannot provide a technical safeguard.

The second point I want to make is that the seismic signals from underground nuclear explosions are believed to be similar to the signals from some earthquakes. On this basis it is assumed that nuclear explosions cannot be identified by their seismic signals alone. Inspection of the site of the suspected event is required for the purpose of obtaining positive proof of the cause of the event. To prove

that a nuclear explosion has taken place it is necessary to obtain a sample of radioactive debris.

Keeping these limitations in mind at all times, we would like to be able to estimate the capability of a control system to detect and identify seismic events in the Soviet Union and in the United States. To make such an estimate, however, it is necessary to have a great deal of information. The detailed location of the control stations must be speicfied. The frequency and distribution of earthquakes must be known. The seismic noise conditions at the control stations must be known—noise interferes with the detection of seismic signals and limits the capability of a control system. Finally, it is necessary to specify a criterion for identifying the signals from a given seismic event as being natural in origin. Available data on all these aspects of the problem are meager and inconclusive.

Some of the uncertainties can be eliminated by a continuing research program in seismology directed at those questions pertinent to the detection problem. Some, however, can only be eliminated through free access to the Soviet Union—specifically, the availability of suitable quiet sites for control stations and the determination of the number of earthquakes occurring within the Soviet Union, particularly the low-magnitude earthquakes.

Some uncertainties, and perhaps the most important ones, concern the nature of the seismic signals from unconcealed underground nuclear explosions. So far only six such underground nuclear explosions have been undertaken by the United States. These experiments have several, possibly vital, limitations. They were all conducted in the same environment, at essentially the same depth below the surface and in essentially the same geological conditions. These limitations are important because the amplitude of the first ground motion, which presently is the only basis for identifying underground seismic events as earthquakes, may depend upon these parameters. They are also important in connection with all the diagnostic aids that have been suggested for discriminating earthquakes from nuclear explosions. We simply do not know at present whether the Nevada explosions signals are characteristic of unconcealed underground explosions or whether they are a very special type of signal which will be different for explosions under other conditions. This uncertainty could be of overriding importance in evaluating the capability of a control system. For in using the characteristics of the Nevada signals as a basis to discriminate earthquakes from nuclear explosions, the possibility exists that all of our conclusions will be proved wrong at some later time. Nevertheless, for the present, that is all we can do, and that is what we have done.

Specifically, we have taken the criteria given in the U.S. Report of Technical Working Group II, which identifies a seismic event as an earthquake provided the peak seismic signal is 20 to 40 times the noise background, and provided first motion rarefactions are observed at four stations. This criterion was believed by the U.S. Working Group II to be a good criteria consistent with the Nevada underground nuclear explosion data. A good criterion must not misidentify nuclear explosions as earthquakes and should identify as many earthquakes as possible. When it came to other uncertainties, such as the frequency of earthquakes in the Soviet Union, all we could do was to make a good guess.

Before presenting the results for the system capability, however, I would like to remind you that the current U.S. proposal to the Conference on the Discontinuance of Nuclear Weapon Tests is for a threshold at magnitude 4.75 which corresponds roughly to the 19-kiloton Blanca explosion. Therefore, our analysis is aimed primarily at the question: how many earthquakes per year of magnitude greater than 4.75 fail to be identified as earthquakes by the control system, and could therefore be suspected of being nuclear explosions. In our first study we assumed that control stations of the Geneva system were located in three countries only, 14 in the United States, 1 in the United Kingdom, and 21 in the Soviet Union. All the stations were assumed to be equipped with the improved seismic instrumentation recommended by Technical Working Group II. The results are: there will be 173 earthquakes above the threshold per year in the United States which are unidentified and 53 per year in the Soviet Union. If the Geneva system of control stations is installed throughout the whole world, the number of such unidentified earthquakes in the United States is reduced to about 143 per year and in the Soviet Union to 28.

If we make the assumption that, say, 30 percent of these unidentified events will be inspected, there will be 52 inspections per year within the United States in the first case, and 43 per year in the second—and 16 inspections per year within the Soviet Union in the first case and 8 per year in the second.

Having estimated the capability of the system in this way, we went back over our analysis, step by step, to see what are the critical assumptions on which the

results most sensitively depend. We found that the criterion for identifying earthquakes was far and away the most important assumption. For this reason we decided to reexamine all the Hardtack seismic data—to be as sure as possible that the criterion used—the criterion of Technical Working Group II—was well-founded.

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Having found a good criterion, we asked the question whether there were some minor changes that could be made to improve the Geneva system. Specifically, we asked the question whether the system could be substantially improved by increasing the number of control stations only slightly.

Our idea here was based on the fact that in the Soviet Union about 90 percent of all earthquakes occur in a small fraction of the total area of the Soviet Union, namely about 5 percent. It seemed plausible that the addition of a few more stations in these seismic areas would make a big improvement. Accordingly we reevaluated the system increasing the number of stations inside the Soviet Union from 21 to 25, not a very big increase. The result of this increase, however, was quite remarkable. Instead of about 50 unidentified events per year, there were now only about 20. Instead of 15 inspections, there were only 6. This is a big improvement for a small cost.

At this point it is natural to ask: How far can one go in this direction? And so we added more stations within the Soviet Union and reevaluated the system each time. Of course, the more stations added, the better the system became. However, the rapid gains stopped after we added another 5 stations, making a total of 30. After that we had to add lots of stations to make small improvements. For the 30-station system we found that 9 earthquakes per year were unidentified above magnitude 4.75 and 36 per year above magnitude 4.35—magnitude 4.35 is equivalent to the 5-kiloton Logan explosion. With 30 stations in the Soviet Union—which is an increase of 9 stations over the assumed Geneva system—we obtained the important result that the capability of the system for identifying earthquakes within the Soviet Union was essentially restored to the capability estimated by the 1958 Conference of Experts.

Representative HOLIFIELD. Dr. Romney, you have a statement here, "Means for Improving Seismic Detection" which you would like to go through. We will take the time for that at this time.

Dr. ROMNEY. Yes, sir. We understand that was the next topic, so if you have no questions for Dr. Latter, I will proceed.

Representative HOLIFLED. I think it is important that we have this information for the record, so you may proceed. Before you do, Mr. Ramey has a question.

Mr. RAMEY. Dr. Latter, you have indicated an improved method of detection. Where does that stand right now? Is it being considered?

Dr. LATTER. Excuse me, sir; I don't understand your point.

Mr. RAMEY. This improved criterion that you are talking about here that would make it easier to add a smaller number of stations,

and so on. Just what is the status of this concept? Has it been proposed to the proper authorities? Is anybody considering it?

Dr. LATTER. This is the result of some work that has just been completed as of a week ago. It is just being communicated at the present time.

Mr. RAMEY. Is this a part of this improved research program?

Dr. LATTER. It is a part of the research program; yes.

Representative HOLIFIELD. We are going into that in some detail later, so we will take it up further. Go ahead, Dr. Romney.

Dr. ROMNEY. Basic problems in detecting underground nuclear explosions: Effects of subterranean disturbances at long range.

The high-energy radiations from nuclear explosions deep underground are rapidly absorbed by the surrounding rock, and most of the energy is converted into heat and compressional elastic waves At each discontinuity in the physical properties of the rock (including the discontinuity at the surface) part of the compressional elastic energy is converted into other types of elastic waves. The result is that all of the known types of seismic waves (i.e., body waves of compressional and shear types, and surface waves of Rayleigh and Love types) are generally observed to propagate from an explosion.

These seismic waves are the only known effects of such explosions which are detectable at distances beyond a few thousand feet from the point of detonation. The amplitude and frequency of the generated waves depend upon such factors as the yield of the explosion, the physical properties of the surrounding medium, the size and shape of the underground shot chamber, the depth of the explosion, the geological conditions and topography of the immediate surrounding area.

At great distances, the characteristics of the seismic waves depend additionally on structural and elastic parameters of the earth through which the waves propagate. Little is known about the possible variations in the characteristics of seismic waves from underground explosions since only a limited number have been fired, all of which were in the same material under very similar circumstances.

Representative HOLIFIELD. Dr. Romney, it seems that our committee has had to go to their various offices to sign their mail. On second thought, I am going to carry this over until the morning. It has been released to the press for such use as they need. We will start with this first thing in the morning at 10 o'clock if you will allow me to change our plans a little bit and go on in the morning. It is 10 minutes after 5 now.

Dr. ROMNEY. Very good, sir.

Representative HOLIFIELD. The committee is adjourned until 10 o'clock in the morning.

(Whereupon, at 5:10 p.m., Tuesday, April 19, 1960, the committee was recessed, to reconvene at 10 a.m., Wednesday, April 20, 1960.)

TECHNICAL ASPECTS OF DETECTION AND INSPECTION CONTROLS OF A NUCLEAR WEAPONS TEST BAN

WEDNESDAY, APRIL 20, 1960

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION AND
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittees met, pursuant to recess, at 10 a.m. in room P–63, the Capitol, Representative Chet Holifield (chairman of the Special Subcommittee on Radiation) presiding.

Present: Present: Representatives Holifield, Durham, Price, Aspinall, Van Zandt, Hosmer, Bates and Westland; Senators Gore, Jackson, and Bennett.

Also present: James T. Ramey, executive director; John T. Conway, assistant director; George F. Murphy, Jr., professional staff member; and Richard T. Lunger, staff consultant, Joint Committee on Atomic Energy.

Representative HOLIFIELD. The committee will be in order.

The Subcommittee on Research and Development and the Special Subcommittee on Radiation jointly meet again this morning in the second day of public hearings on the technical problems of a nuclear weapons test ban and the necessary detection techniques and inspection controls.

Yesterday, at the opening of these hearings, I explained the purpose of the hearings and what we intended to cover. In order that there may be no misunderstanding, I will reiterate this morning what I stated yesterday:

I should like to emphasize that these hearings will be concerned primarily with the technical aspects of a test ban. We desire to avoid as much as possible any discussion of policy questions as to whether a test ban fits into a desirable system of disarmament.

There may be policy questions on a test detection system as such which we should and must discuss. Such discussions, however, should be confined to the technical possibilities of the problem, if possible.

We also hope to avoid emotional or philosophic discussion of the effects of nuclear testing in terms of radioactive fallout. The technical aspects of the effects of fallout have been considered rather extensively in previous hearings.

In line with this policy which was adopted by the committee and its procedure, the committee has just met in executive session and they have decided that we will invite Ambassador Wadsworth to appear before the committee at 1:30 this afternoon in executive session as matters of policy might be discussed, and we have no desire to bring

that into the hearings. So he will meet with us at 1:30 and we will resume hearings in this room at 2:30 this afternoon.

As I also explained yesterday, it is important that all presently available facts on this subject be brought to the attention of the Congress and the public. If and when a treaty is proposed, we should have an informed Congress and public capable of understanding what a treaty involves.

Also, if we are to succeed in having an effective control system, it is important that we know and understand what the research program may include to improve the detection system. We should know this now so we can begin this research as soon as possible.

On our first day of hearings, Dr. Harold Brown and Dr. Wolfgang Panofsky reviewed the history and results of the Conference of Experts and the Technical Working Groups I and II whereby technical and scientific representatives of the East and West have attempted to evolve agreed-upon scientific data for formation of a control and inspection system.

Dr. Carl Romney and Dr. Richard Latter also began a discussion of the Geneva-type international control system possible under our present state of knowledge. They reviewed the capabilities and limitations of detecting underground tests.

At the time we recessed, Dr. Carl Romney was discussing various means to improve the system. We will resume this morning with Dr. Romney who, on request of the committee, has with him this morning a seismic instrument which we hope may be of assistance to the committee and to the public in understanding the use of this instrument in the detection and identification of nuclear tests.

Dr. Romney, if you are ready to resume, we will begin.

STATEMENTS OF DR. CARL ROMNEY,[1] AIR FORCE TECHNICAL APPLICATIONS CENTER; AND DR. RICHARD LATTER,[1] THE RAND CORP.—Resumed

Dr. ROMNEY. Thank you, Mr. Chairman.

Yesterday I summarized very briefly some of the effects from underground nuclear explosions which are detectable at long ranges. Part 2 deals with detection.

Seismic waves are detected by measuring the displacement or strain along the surface of the earth. Although the motions are minute—of the order of 10 angstroms at 1 cycle per second at a distance of 1,000 kilometers from 1 kiloton—instrumental techniques are sufficiently well developed so that these motions can be measured with little trouble.

I have here and would like to demonstrate for you a simple seismograph. This instrument here is a small version of the Benioff seismograph as used on the Hardtack experiments. It consists of a mass suspended by a spring and containing a set of coils. There is also a magnet which is attached to the earth.

The mass which is suspended on a spring tends to remain fixed with respect to the earth. As the earth moves the case and the suspending mechanism, the mass tends to stand still. The earth goes up and down, but the mass tends to stand still.

[1] See biographical sketches p. 88.

I can drive the seismograph. You can see what happens. Very small motions of the moving part are converted into small electrical signals which are recorded, then, on one of several mechanisms.

The specific device we have here is a heated stylus recorder. If the earth receives a sudden jolt due to seismic waves from an earthquake or an explosion, then a very small signal is generated and recorded.

Unfortunately, this room is so noisy that the seismograph is overloaded at all times, so I cannot really show you how it works. It is designed to detect motions of thousands of a millimeter and this room is probably moving at appreciable fractions of a millimeter so that the instrument is overloaded.

Representative HOLIFIELD. In other words, the background noise is so much in this room that it would obscure any normal shock that might occur on the table there?

Dr. ROMNEY. That is correct. This illustrates the basic problem in detection which is not sensitivity of the equipment itself, but rather, the problem that the noise in the earth is very large compared to the signals we are trying to detect.

Representative HOLIFIELD. What does the normal noise level reach in relation to the 4.75 reading which would occur ordinarily? Is it half of it or a third of it? What percentage would it ordinarily be? I understand it would vary in different parts of the earth.

Dr. ROMNEY. Yes, sir. This also depends on the distance because the seismic waves on the blast decrease with distance. So at small distances like 700 kilometers—400 miles—the motions from a bomb of 20 kilotons in Rainier conditions would be expected to be appreciably larger than the noise in the earth.

Representative HOLIFIELD. Would you say twice or three times as large?

Dr. ROMNEY. At 700 kilometers it would be something like 10 times.

Representative DURHAM. Doctor, that means a lot of machines and a lot of stations to listen on whatever happens.

Dr. ROMNEY. At that distance; yes, sir, it would.

Representative DURHAM. That is the distance they are discussing over there, 700 kilometers.

Dr. ROMNEY. The distance being discussed at Geneva now is stations spaced 1,700 kilometers apart in most of the world and 1,000 kilometers in the seismic areas of the earth. That is in the areas where most of the earthquakes occur.

Representative DURHAM. This machine is good for how many kilometers?

Dr. ROMNEY. This particular machine could detect such an explosion at distances of the order of, say, 800 kilometers with quite a good signal-to-noise ratio, provided you carefully selected your operating station sites.

Representative DURHAM. Is there any machine that reaches out farther in detecting?

Dr. ROMNEY. This machine will also detect signals, although at a lower signal-to-noise ratio—that is less useful signals—at greater distances, particularly in the distance range from about 2,000 kilometers to 4,000 kilometers.

Representative HOLIFIELD. Could you tell us if this was the type instrument that was used in College, Alaska, that detected the Nevada site Rainer explosion?

Dr. ROMNEY. Yes. The instrument used at College, Alaska, is essentially identical to this. It is, however, larger and has slightly greater sensitivity than this particular instrument. This is a small version of that instrument.

Representative HOLIFIELD. That detection was a distance of about 3,000 miles, was it not?

Dr. ROMNEY. That was 3,700 kilometers, which is 2,300 miles, I believe.

Representative HOLIFIELD. So the factor of distance is not the only determining factor. Background noise and the type of transference through the strata of the earth, I suppose, has an effect upon the reading.

Dr. ROMNEY. That is correct. The point of significance here, I believe, is that detection with sufficiently good signals to determine the location of the source can be accomplished at very great distances.

Representative HOLIFIELD. Providing you know in advance that it is coming and that the test is taking place, or regardless of whether you know it?

Dr. ROMNEY. Regardless of whether you know it, if we talk about signals of the size of Blanca, the 19-kiloton shot.

Representative HOLIFIELD. The 19-kiloton shot was detected at College, Alaska, 2,300 miles away. Was that station alerted to the fact that a shot was going to take place?

Dr. ROMNEY. Yes; they knew the shot was going to take place. However, the signal was easily recognizable. They would not have had to know.

Representative HOLIFIELD. It was easily recognizable in that the markings were much higher than the background noise at that particular place. At that distance would you have been able to have identified from the reading the explosion?

Dr. ROMNEY. The signal reported at that distance was marginal for identification purposes. I, myself, felt that it was a useful signal. Some of my colleagues who have examined the same record do not feel it was useful. It is a very gray area.

Representative HOLIFIELD. In other words, the initial movement of the recording needle in your opinion was not quite strong enough to indicate that it was a down movement?

Dr. ROMNEY. I would say it was just barely strong enough. Other people would say it was not quite strong enough.

Representative HOLIFIELD. If there are no further questions, you may proceed.

Dr. ROMNEY. In this part of the spectrum, the limit to detection is thus imposed by natural microseismic noises in the earth. The properties of the microseismic noise are not well known, although it is possible to make general statements about amplitudes in the 0.1 to 10 cycles-per-second range.

However, since the observed noise depends on the intensity of the source, the distance, and the path of propagation, the amplitude is found to be highly variable from place to place and from time to time. At 1 cycle per second, for example, noise amplitudes of the order of 10 Angstroms are observed under extremely quiet conditions, and amplitudes of 200 angstroms and greater are observed under noisy conditions.

I believe in this room our noise level is probably in excess of 1,000 angstroms. More detailed knowledge is required on the spectrum and coherence of noise, particularly at other frequencies, for complete understanding of the detection problem.

3. Identification: A far more serious problem than the detection of seismic waves is the problem of identifying the nature of the source. This is one of the classic problems of seismology, which to date has been solved only under very specialized conditions.

The seriousness of this problem is apparent when it is considered that kiloton-size bombs produce waves equivalent to and under most circumstances indistinguishable from very small and very numerous earthquakes. Thus, at the present time it is possible only to classify seismic disturbances into two categories:

(*a*) Identified earthquakes.

(*b*) Unidentified earthquakes and explosions.

Since the final identification of an underground nuclear explosion requires obtaining a sample of nuclear debris through on-site inspection, the difficulty of such identification is directly proportional to the number of seismic disturbances in the second group. The most significant area of potential improvement is, therefore, to be found in the development of techniques for identifying the nature of seismic sources. I say this to distinguish investigations in this area from simple improvements to instrumentation.

4. Concealment: A variety of methods are available if special efforts are made to conceal an underground nuclear explosion. These include the use of noise cover from large earthquakes and aftershock sequences which are observed to follow such earthquakes, the placement of the bomb in a position where a given seismic network has a minimum detection capability, and the firing of multiple-explosive charges to produce confusing signals. These are all difficult.

However, on the basis of theoretical analyses, by far the most effective method appears to be that of reducing the size of the distant seismic signal by suitable design of the shot chamber. The effectiveness of the latter method for decoupling seismic waves has been amply demonstrated, at least for high explosive sources of the order of 1 ton, by the recent experiments in Louisiana (Operation Cowboy).

II. IMPROVEMENT TECHNIQUES

The most complete study of seismic improvement techniques was conducted by the Panel on Seismic Improvement, Dr. Lloyd V. Berkner, chairman.[2] The results of this study have received wide distribution and do not require discussion in detail.[2]

Briefly, however, the panel concluded that the present difficulties in the detection and identification of small seismic events are fundamental in nature and, therefore, the solution to these problems requires a fundamental attack on a broad range of seismological problems to be followed by a development program for translating research work into a specific detection system.

Because of the fundamental nature of the problem, therefore, I will speak chiefly of what might be done through research rather than of what we are now sure we can do. However, there are certain techniques which we can now use and I will mention those also.

[2] Berkner Panel report, app. 5, p. 643.

Representative HOLIFIELD. Senator Jackson?

Senator JACKSON. This point may have been covered, but, Doctor, I want to ask you, in your judgment is the art of concealment improving faster than the art of detection, and will it in the future?

Dr. ROMNEY. This is an extremely difficult question. The art of concealment in the past couple of years certainly has advanced far faster than the detection improvements. Many people feel that this could well continue for some time. On balance, I find it difficult, myself, to forecast results.

Senator JACKSON. Up to now it is quite clear, is it not, that the ability to conceal has been demonstrated well beyond that which the experts, or some experts, have believed it to be possible? This is correct, is it not?

Dr. ROMNEY. Yes.

Senator JACKSON. If they are able to come up with various methods and approaches to concealment that makes it impossible for distant detection devices to pick up, short of onsite inspection, your approach from the standpoint of instrumentation simply will not cover the field: is that not a fair statement?

Dr. ROMNEY. I think that is reasonably fair. I would like to say one more thing in response to your question, however.

In terms of a specific, fixed network of stations, I believe the concealment can proceed at least as fast as the improvement techniques. However, if you will allow, as a means of improvement, the addition of new stations to the detection system, then I am not sure what the answer is.

Senator JACKSON. What I am getting at, based on what has happened to date, it would seem to me as a layman to point up the need for more stations because with the state of the art in concealment improving up to now faster than the art of detection, just to be on the safe side it follows, does it not, that you would have to have in any event substantial numbers of stations to make certain that the art of detection could keep pace to a certain extent with the art of concealment.

Dr. LATTER. Perhaps I could make some comments on your point.

I really think there are two points at stake here. One is the question of whether detection methods will improve faster than concealment methods will improve. I think there is in fact a technical reason to believe that concealment will improve, or the capability to conceal will increase, more rapidly than detection methods.

There is a genuine technical basis for this statement which perhaps I can give you at this time. That basis is as follows: Suppose that concealment methods improved by a factor of 10, so that by concealment a 10-kiloton explosion could be made to look like 1 kiloton.

We would then be faced with the problem of improving the system to have a capability of detecting events at 1 kiloton. What is usually meant by the capability of the system is that a certain percentage of the events observed by the system are unidentified. For example, 90 percent, which leaves 10 percent unidentified. If we had at the 10-kiloton level a total of 100 events, 10 percent unidentified is 10. We are now faced with the problem of improving the system to account for the factor of 10 increase in concealment.

Suppose we managed to improve the system to a point where again at 1 kiloton it had a 90-percent capability. This would be a factor

of 10 improvement. We are still left with 10 percent of all events unidentified. However, at 1 kiloton the number of events that we must now consider is 10 times larger. Instead of 100 it is 1,000. So 10 percent now means 100 events.

The improvement we need is another factor of 10 to get back to the same number of unidentified events as we had at 10 kilotons.

Senator JACKSON. What you are saying, if I understand it correctly, is that as the state of the art of concealment improves very rapidly, the problem that you face is not just detection, but the complicating problems of identification that are created in given situations.

Dr. LATTER. You are also facing the problem that at the lower yields that you might be attempting to detect and identify there are a greater number of confusing signals. Not only must you for a given capability of the system change the yield at which you achieve that capability, but you must in fact increase the capability.

So a factor of 10 improvement in concealment would require a factor of 100 improvement in detection capability.

Senator JACKSON. In other words, there are certain areas of disturbances which occur more frequently, and of a certain kind—that is, you can just about predict the kind that go on with natural occurrences in earth—so that when you set off a nuclear explosion that can come within that realm you place a heavy burden of identification upon the detection and identification experts.

Dr. LATTER. As you try to push the level of the yield at which you detect and identify lower, you are faced with many more confusing signals.

Senator JACKSON. So when we are talking here, and the public reads that a yield below 20 kilotons is probably not detectable and identifiable, next year it could be 100 or 300 kilotons. I am just giving this as a figure of speech at this point, but it is certainly a possibility if the art of concealment continues to improve.

Dr. LATTER. To answer that specific point, I think we already believe and you will hear more about this in connection with the decoupling method, that explosions in the range of maybe hundreds of kilotons already can have their signals reduced to below the level of a 20-kiloton explosion. That is already possible.

Senator JACKSON. That certainly explodes the publicity that has been given out to the effect that what we are really talking about is the area of yield of 20 kilotons and below, Mr. Chairman.

I am sorry. I did not mean to depart from this point, but I wanted to ask these questions.

Dr. LATTER. There was an additional point you raised, however, on the addition of stations as a means of improvement. Yesterday I spoke to this point and indicated that by the addition of a small number of stations one could, in fact, obtain considerable improvements in the system.

But I also pointed out that at some stage after you add a specific number of stations in the Soviet Union—for example, nine—the improvements you make after that are small. In order to obtain appreciable further improvements you need many, many stations.

Senator JACKSON. I can see what the trend is going to be. As the art of concealment improves, you are going to have to step up the number of detection stations with no assurance that you will necessarily achieve 100-percent coverage.

Dr. LATTER. You are going to have to step it up more rapidly than the improvements in the concealment.

Senator JACKSON. That is right. I understand that.

Thank you, Mr. Chairman.

Representative HOLIFIELD. Dr. Romney, at this point I want to read to you from the Technical Working Group No. II paper, page 3, part of the Soviet annex, which is critical of our procedures in the Hardtack series. I read as follows:

> The document of January 5 and the report to the Conference give the impression of a large-scale, well-conducted experiment. In actual fact, the situation was quite different. It was ascertained during the meeting of the group that of the dozens of seismic stations in the United States, only a small proportion possessed instruments that satisfied, if not the recommendation of the Conference of Experts, even the elementary conditions demanded of any scientific instrument.
>
> For example, the Logan explosion was registered by 54 seismic stations in the United States. However, about only one-half of them, 28, had properly calibrated instruments. In the opinion of the U.S. seismologists themselves, seismograms from only 16 of these 28 stations could be used for analysis.
>
> It is indeed surprising that the instrumentation was not calibrated at least for the period during which these unique experiments were to be conducted.

This is their criticism and we would invite your comment on that. We uncovered the fact yesterday of the description of the instrument so you might devote your answer at this time to the charge that they were not calibrated.

Dr. ROMNEY. Yes. This criticism seems to be of the fact that in the United States we have a rather great amount of freedom. The Government of the United States cannot demand that a university or some private operating group operate its stations in a particular manner. I hope it never gets to that point.

This is precisely the case, or precisely the facts. Many of the stations which reported the Hardtack experiments were stations operated by private groups. It is not true, as the Soviets allege, that these data were useless for analysis. They were useless for certain kinds of analysis, but they were very useful for other kinds of analysis and we did use them for that.

Our statement to the Soviets in Geneva was that only a limited number of them had the precision of calibration necessary for measuring precisely the size of the seismic waves. But this is just one small problem in association with detection.

Representative HOLIFIELD. Would you furnish for the record the names of the 16 stations and their approximate distances from the Nevada test so that we might include that in the printing?

Dr. ROMNEY. I certainly will. I will add that at some later time. (See attachment 3, p. 425.)

Representative HOLIFIELD. The staffs suggests that we ask this question: These stations were not under contract to the United States?

Dr. ROMNEY. The uncalibrated stations were not.

Representative HOLIFIELD. They were all independent stations and their assistance was on a voluntary basis?

Dr. ROMNEY. That is correct. They are stations where instruments are operated normally every day, 365 days a year.

Representative HOLIFIELD. And you consider that 16 stations reporting on that particular shot was an adequate—16 stations were calibrated instruments, I should say—coverage?

Dr. ROMNEY. We would like to have had 100. The 16 stations were stations especially installed by the Government for making these

measurements. The limitation was on the number of seismometers available to the Government for this purpose.

Representative HOLIFIELD. Were they placed at various distances?

Dr. ROMNEY. Yes, sir.

Representative HOLIFIELD. What was the range of distances?

Dr. ROMNEY. They ran from a distance of around 40 kilometers from the test site along a line which followed Highway 66 generally east, and then northward into Maine. The maximum distance was 4,020 kilometers from the test site.

Representative HOLIFIELD. It was representative from the standpoint of distance?

Dr. ROMNEY. It was a very good experiment, I think, considering the limitations of equipment and time.

Representative DURHAM. Doctor, since you made that statement, the identification would have been better if you had 100 stations; is that correct?

Dr. ROMNEY. We would have learned a great deal more about how to detect explosions. Of course, you can never satisfy a scientist, and I would have been very happy with 50 as well as with 100.

Representative DURHAM. Then, of course, we have something here that is very important if we are going to reach a disarmament agreement for the civilized world. I think it is important that we know the facts.

We throw millions of dollars around for most anything that comes along and I consider this one of the most important things facing the American people and the civilized world today. The thing that worries me a little bit, and I do not care to get into this, Mr. Chairman, at the present time, but we have been talking about 40 identification stations in Russia.

Reading between the lines of your statement, I am not sure it would be worth anything from the point of view of identification. That is the way I have felt all along. You do not have to comment on that.

Representative HOLIFIELD. You may proceed, Dr. Romney.

Dr. ROMNEY. Specific areas of research which can be expected to lead to improvements are as follows:

1. Additional stations: An immediately available technique for improving the capability of the control system is through the addition of stations to the control network.

Dr. Latter talked yesterday about the improvements that can be made with the addition of a very few stations, and he reconfirmed that today.

The Conference of Experts also recognized the value of that approach as indicated by the statement in their final report to the effect that—

> the dependence between these parameters is such that with an increase in the yield of the explosion of the number of control posts the probability of detection and identification increases, and the number of unidentified events suspected of being a nuclear explosion decreases.
>
> On the other hand, for the identification of the increased number of unidentified events resulting from a smaller number of control posts, it would be necessary to increase the number of onsite inspections or to make greater use of information coming from sources not subordinate to the international control organ or, if necessary, both.

Since many of the detection problems result from the fact that signals of interest may be lost in noise, and since the amplitude of seismic waves decreases rather rapidly with distance, an increase in the num-

ber of stations means that the average detected signal will be larger compared to noise and, therefore, more usable for identifying the probable nature of the source. Three specific proposals have been made:

(*a*) The first proposal is to increase the number of control posts of the detection system. In addition to the seismic improvement, this approach has the merit of reducing the threshold of detectability for air shots as well.

(*b*) The second approach is to add a number of simplified manned seismic stations to the network. These first two approaches have the political disadvantage of placing additional men in foreign countries where they will not necessarily be welcome. Their technical advantage, however, has been demonstrated by Dr. Latter.

(*c*) The third approach involves the addition of extremely simple unmanned seismic stations to the control network and providing means for telemetering the data to the manned control posts. The disadvantages of this approach are the high cost of installation and operation—of the order of $100,000 initial cost per station and a somewhat lesser cost annually for operation, and a substantial part for communication and security—problems associated with the physical security of the station and of the integrity of the data, and perhaps the problem that this approach requires far greater access to foreign territories than do manned stations because of the greatly increased number of stations required—the most authoritative study, by the Berkner panel, suggests that the stations should be spaced on the order of 100 miles apart. Advantages of this approach are the greatly increased amount of data from closely coupled explosions, and the expectation that it might be possible to use such a fine spacing that even decoupled shots of a few kilotons could be detected.*

Representative HOLIFIELD. What has been the attitude of the Soviets toward the so-called unmanned or robot stations?

Dr. ROMNEY. I am not familiar with this, sir.

Representative HOLIFIELD. Let me ask you this question: Would there be any possible way of dampening the unmanned stations at the time of impact through deliberate methods to obstruct them?

Dr. ROMNEY. It is rather difficult to do things in such a way that you do not leave some trace. You can break into the station and cut the electrical power off.

Representative HOLIFIELD. That would show a break in the recording, and if the regular background noise continuity was interrupted, that would also show a break on the recording machine, would it not?

Dr. ROMNEY. It might or it might not. There are possibilities of increasing the noise in the vicinity of the stations. I cannot be very specific about this, but you can produce vibrations near one of these stations and interfere somewhat with the detection.

However, you would have to do this at a great many stations, and it might appear to be a little singular that a whole district full of stations suddenly became exceedingly noisy. On the other hand, I do not know what you do about it if this did happen.

Representative HOLIFIELD. Have you had any experience with unmanned seismograph stations?

Dr. ROMNEY. Some minor experience. We have not investigated the problems in detail of making the stations completely tamper proof. However, from a technical standpoint—a technological stand-

*See also, Sandia "Study of Unmanned Seismic Stations in the U.S.S.R.," app. 6, p. 839.

point, rather—I think it is a rather simple job at about $100,000 per station, including communications. But it is straightforward, state-of-the-art business of constructing them.

Representative HOLIFIELD. This would have to depend upon a political agreement to establish these stations. I suppose it would also depend upon regular inspections of the stations to see that they were functioning properly and to pick up the recording sheets and so forth.

Dr. ROMNEY. Yes, sir.

Representative HOLIFIELD. This would involve additional people inside the borders of these countries, would it not?

Dr. ROMNEY. I believe this would be the case; yes. There is also the possibility of somehow reversing the polarity of the unmanned station. This would be difficult. It would require access at some stage to the detector itself or to the communication system and would change the compressional motion from a bomb into a rarefactional motion leading to the interpretation that it was an earthquake.

This, I say, could be done. The feasibility of it really would depend in great detail on how well the stations were constructed and the kind of communications provided for the stations.

Representative HOLIFIELD. Mr. Ramey has a question.

Mr. RAMEY. An advisory council to a national organization has recommended some improvements to the Geneva system. I would like to get your comments on them. It reads as follows:

> The principle change required in the Geneva system is a reduction of the grid spacing to 400 kilometers or less. This could be achieved either by adding more manned stations to the grid or by filling in the gaps with unmanned or robot monitors.
>
> This closer spacing of monitors would permit (*a*) detection on the seismic signal of a 3-kiloton test even though this explosion was thoroughly decoupled; (*b*) more precise location of seismic signal origins; (*c*) the possibility of discriminating against most earthquakes by an unambiguous observation of the direction of first motion.

Dr. ROMNEY. Dr. Latter has studied this in detail.

Dr. LATTER. I would like to make some comments on that.

We have looked at the problem of setting up a network of stations spaced at 400 kilometers. We have looked at the capability that the system would have to detect and to identify earthquakes. This problem is somewhat difficult as I described yesterday.

One must know the precise locations at which he is going to place stations. One must know the noise conditions of the stations, and the instrumentation that will be placed at the stations.

We had to make a number of guesses on these conditions in order to study this problem. We have taken some very optimistic estimates to see just how far we might go with such a system. We assumed the stations were exceedingly quiet—as quiet as could be obtained by very careful selection of sites. We have even assumed that at each station there were 100 seismographs as recommended by Technical Working Group II, which is a very large number of seismographs.

Representative HOLIFIELD. Why should there be a hundred seismographs? Are the instruments that undependable?

Dr. LATTER. No. The point is to reduce the noise background so hat the signals are more readable.

Representative HOLIFIELD. Why does that reduce the noise background?

Dr. LATTER. By connecting together the seismographs in an array it is known that the noise background will be reduced. It is a physical fact. Dr. Romney will be talking more to this point in a little while. This was one of the recommendations of Technical Working Group II, to go to the 100 seismograph arrays in order to reduce the noise.

So we looked at a very optimistic situation. Quiet sites, 100 seismograph arrays of the most improved sort. We note that the number of stations that would have to be in the Soviet Union under such a grid spacing is approximately 190 as opposed to the kind of numbers that are thought about for the Geneva system, which are 20 or so, and the kind of numbers which I talked about yesterday for improving the system, of 30.

In this case one finds that the limit of detection for this system with a 400 kilometer grid spacing is 18 tons. But at this level of detection there are possibly 19,000 events which will be observed by the system in the Soviet Union alone. Of these events, 13,000 are unidentified. That is a very large number to be faced with when trying to make a feasible detection system.

Representative DURHAM. How did you arrive at those figures, Doctor?

Dr. LATTER. These figures were based on the data from the Hardtack series, the amplitude of the seismic signals from the nuclear explosions. They were based on the estimates of the seismicity in the Soviet Union—how many earthquakes occur in the Soviet Union. They were based on the predictions of the capability of 100-seismograph arrays.

The number of stations involved is simply based on the area of the Soviet Union and how many you need to place within the Soviet Union.

Representative DURHAM. In other words, repeat the Hardtack series, put them in Russia, and say this is the number.

Dr. LATTER. That is right. From the Hardtack series you can determine what the signal from an 18-ton explosion is by extrapolation.

Representative DURHAM. Did you get into the question of the cost of such a detection system?

Dr. LATTER. I did not get into the cost of it. Perhaps Dr. Romney can speak to that.

Representative HOLIFIELD. We are getting into that later. We have an item on the agenda later.

Dr. LATTER. I think if you are concerned about cost, it is important to look at the question of reducing the number of seismographs at each station because clearly this is a very large number of seismographs. It is about 20,000.

Representative HOLIFIELD. I think this question is important enough. We are spending $40 billion a year for armament. We could possibly siphon off a few million dollars from that to establish these stations. I think when we get into the cost of establishing stations and compare that cost with the cost of our war effort on an annual basis, I do not think it will be such an astounding cost.

Dr. LATTER. I think it may be interesting, though, to see what the capability of the system would be if you installed one seismograph at each station. This will enable you to normalize.

Representative HOLIFIELD. I want to ask you this question: Assuming that we have 21 stations for Russia——

Dr. LATTER. That is a number——

Representative HOLIFIELD. That is a number which has been discussed.

Dr. LATTER. It has not been agreed to.

Representative HOLIFIELD. I understand it has not been agreed to. Assuming each one of these stations would have 100 seismographs, then you would have some 2,100 seismographs for recording purposes within the Soviet Union.

Now, getting back to the Hardtack series, we calibrated the Hardtack series with 58 stations, I believe, and how many seismographic instruments did you have in total at the 58 stations?

Dr. ROMNEY. At most of the stations there were three component instruments; that is, one instrument which responded to vertical motion, one which responded to motion in a north-south direction, one which responded to motion in an east-west direction.

At one of the stations there was in addition an array of four vertical seismographs for noise cancellation purposes. This station was operated for all of the Hardtack shots.

Representative HOLIFIELD. Also, if we allow an average of three to the 58 stations, with a few extra, or even four, we would have less than 200 instruments registering on the Hardtack series at varying distances up to 4,000 kilometers; is that right?

Dr. ROMNEY. Yes; that is right.

Representative HOLIFIELD. You seem to be pretty confident of your reading on some 200 instruments. Why do you feel it is necessary to have 2,000? Is it because you would cover a larger territory, or what is it?

Dr. LATTER. Sir, we are trying to detect a smaller explosion. We are trying to detect smaller explosions and identify earthquakes. We are trying to determine the number of unidentified events at any given level. If you try to push that level of explosion down at which you want to detect and identify earthquakes, you must go to a larger number of seismographs.

Representative HOLIFIELD. Then you are talking about going below 4.75 readings when you talk about 2,000 seismographs.

Dr. LATTER. No. The Hardtack data indicated that, in fact, at the level of 4.75 the number of unidentified events in the Soviet Union would be approximately 50 when observed on the system of 2,000 seismographs.

That combined with a requirement that 30 percent of the unidentified events in the Soviet Union would be inspectable led to 20 inspections per year in the Soviet Union. That was the basis for the choice of the 4.75.

Representative HOLIFIELD. Mr. Conway calls to my attention the fact that you talked several times of 50 to 60 events that might register 4.75 whereas our proposal is 100. It seems that there needs to be some clarification.

Dr. LATTER. I believe our proposal is based on the estimate that in the Soviet Union there would be a total number of events—earthquakes—equal to 100 per year. Of these, approximately 60 percent would be unidentified.

Representative HOLIFIELD. I think the proposal said 70 percent would be unidentified.

Dr. LATTER. It is 60 to 70. Our numbers indicate 60.

Mr. CONWAY. We had 30 percent of the unidentifiable and the 20 number was the 20 percent of the 100.

Dr. LATTER. That is correct.

Representative HOLIFIELD. So the fact that you have used slightly different numbers is not significant.

Dr. LATTER. The numbers are essentially the same.

Representative HOLIFIELD. Proceed, Dr. Romney.

Dr. ROMNEY. In addition to adding stations to the system, there is good reason to believe that the response characteristics of seismographs can be improved and be made more adjustable so that signals can be more effectively detected in the presence of noise.

Additional work is clearly indicated at high frequencies—about 10 cycles per second—and at very low frequencies—0.1 to 0.01 cycles per second.

A second method requiring further research is in the use of very large arrays of seismometers, perhaps with as many as 100 detectors. The effect of an array is to reduce the response to incoherent noise relative to the response to coherent signals. What this means in simple language is that with seismometers spaced at intervals along the ground, you would expect that the noise present in the ground will cause one seismometer to be moving up at the same time another one is moving down.

On the average there will be some cancellation, therefore, to the noise. You will not get as an output 100 times the noise because some detectors are giving responses opposite to other detectors. On the other hand, a signal will arrive virtually simultaneously at all of the detectors and in that case all of the detectors will move up at the same time or down at the same time.

So this effect, then, is to make the signal more clearly recognizable in the presence of the noise. In conjunction with the investigation of arrays, studies should be made of advanced data-processing techniques for improving the signal-to-noise ratio by an amount potentially greater than that resulting from the simple additive methods used to date.

A third approach which has been under active investigation for several years is the development of detectors for operation in deep wells several thousand feet below the surface of the earth. Since it is expected that much of the troublesome seismic noise is confined to the near-surface region of the earth, such detectors may be able to reveal signals far smaller than can be detected at the surface.

Here again this is simply a technique for reducing the noise without affecting the signal.

Representative HOLIFIELD. Have the seismologists put these seismographs in deep holes as yet?

Dr. ROMNEY. There has been a program going since about 1954, not quite continuously all the time, seismographs with low frequency response having been operated at depths of 9,700 feet and shallower. The results to date are encouraging, but there are still major engineering problems to overcome.

One of the problems is that there does not seem to be any signal cable capable of withstanding the pressures and temperatures and corrosive chemical reactions of the fluids in wells over long periods of time. The cables break down. Other problems are in connecting the seismometer to the wall of the hole. If the seismometer is simply

floating in the mud in the bottom of the well it does not respond to earth motion the way it should.

So you have to have some kind of "hands" that reach out and grab the rock, or posts that stick into the rock.

Representative HOLIFIELD. What about the many dry oil well holes?

Dr. ROMNEY. Dry holes are dry with respect to oil, but not with respect to water.

Representative HOLIFIELD. If they have been cemented off from drilling operations, the water is kept out of the wells. I am sure you can find dry holes. I do not know whether the size of the casing would allow you to put that size of seismometer in or not.

I know in some of them the casing is larger than that and goes down to a distance of 3,000 or 4,000 feet. I happen to have an oilfield in my district. I know a little something about that. I do not know whether you have smaller sizes of seismographs or not.

Dr. ROMNEY. Yes, sir. The instruments that go into the well are 4 inches in diameter.

Representative HOLIFIELD. If the seismograph is suspended freely would projections from it against the well casing serve the purpose?

Dr. ROMNEY. Yes. You have a motor which drives out some little gadgets which hook into the rock. A cased well, however, has been found to be unsatisfactory for this purpose. The casing itself, apparently conducts the noise from the surface down into the well. We are operating in uncased wells and this is a very nasty problem, as you probably know.

Representative HOLIFIELD. Yes, I can well understand you would have plenty of water and mud problems.

Dr. ROMNEY. Yes. In addition to these equipment-type improvements, there is a need for improved analytical methods. When seismic waves are propagated through the earth, they are subjected to great distortion. The result is that the recorded signals are more clearly characteristic of the structural and mechanical properties of the earth rather than of the source.

This is the basis for seismic prospecting for oil. The signals tell you very much about the earth, but very little about the source. In our particular case we do not want to know about the earth; we want to know about the source. It is a little unfortunate that the information is not very handy.

Representative HOLIFIELD. I might say we have an expert consultant coming, Mr. L. P. Meade, from Phillips Petroleum Co., who is going to testify next on the feasibility of construction of large cavities and the cost and time scale. We understand that the oil companies have had a lot of experience in using these instruments.

Dr. ROMNEY. Yes, sir.

Representative HOLIFIELD. You may proceed.

Dr. ROMNEY. However, as seismology develops to the point where the causes of these distortions are well understood, it will be possible to remove the effect of the earth from the signals and, therefore, recover information which is now lost regarding the nature of the source.

Studies of long-period seismic waves are particularly encouraging in this respect. It has recently been possible to determine the faulting mechanism of a number of large earthquakes through surface wave

analysis. If this technique is developed to the point where similar analyses can be made on small earthquakes, it will be possible to make significant reductions in the number of unidentified events.

Similar analyses can be made of characteristics of the longitudinal waves with the possible result that criteria in addition to the first-motion criterion will be developed. A likely result of continued research will be the development of improved means for determining the depth of focus of earthquakes at depths less than the present limit of about 60 kilometers.

Such a technique would make a significant reduction in the number of unidentified events by establishing their depths below the limit obtainable by drilling techniques. Spectral analysis of the seismic waves from earthquakes and explosions may reveal significant differences. Experimental measurements of such spectra will, of course, have to be supported by active theoretical investigation of the phenomenology of underground explosions and also of wave generation by faulting sources.

I imagine you would all like to know what this kind of research will result in in a reasonable period of time. The program I have discussed, or the kind of things I have discussed, is based heavily on the recommendations of the Berkner Panel.* They suggested expenditure at about $30 million per year, which would include $12 million for large nuclear and chemical explosions, and they suggested that such a program can be expected to result in important improvements in ability to detect and identify earthquakes of small magnitude.

Although the forecasts of results of scientific research are intrinsically speculative, the Berkner Panel concluded that in 3 years the research program would "probably result in further improvements which could achieve the same capability in the Geneva net of 180 stations as was originally estimated by the Geneva Conference of Experts."

This estimate, then, suggests that the control system can be improved to the point where there will remain 20 to 100 unidentified seismic events annually on the continents of the earth.

This does not include any improvement due to adding unmanned stations or additional stations to the network. At the same time, there seems to be no reason to doubt that more effective concealment techniques will be developed. It should, therefore, be recognized that the essential nature of the control problem will remain unchanged, that is, a system consisting of a given number of control stations can be made more effective, but there will always remain a threshold below which nuclear weapons can be tested with little risk of detection.

The yield corresponding to the control system's threshold will be reduced in some proportion to the amount of effort applied to the development of detection methods and increased in some proportion to the amount of effort applied to concealment.

I believe both Dr. Latter and I have given our thoughts on which technique will be improved most rapidly.

Representative HOLIFIELD. In view of your testimony today, would you say you are a little more optimistic on the chances of improving detection techniques than Dr. Brown was yesterday?

Dr. ROMNEY. I do not recall in detail how optimistic Dr. Brown was.

Representative HOLIFIELD. He was not very optimistic, I might say, if you did not hear his testimony.

*See app.5, p. 643.

Dr. ROMNEY. I feel certain significant results, and important results, in connection with the control problem will come out of a vigorous research program. On the other hand, I also feel that you will always be able to hide explosions of some kind.

Representative HOLIFIELD. We are going to go into the status of the research and development program both as to the type of program that is involved and also the cost of it.

From your knowledge at this time, would you say that the program approaches this recommendation of the Berkner Panel or is it lagging in funds?

Dr. ROMNEY. The present program?

Representative HOLIFIELD. Yes.

Dr. ROMNEY. The present program is being conducted at a slightly reduced level. The principal difference between that recommended by the Berkner Panel and the program actually underway is that the number of shots—large explosions, that is—have been reduced, and this is because of rather obvious political problems in connection with firing nuclear explosions.

Representative HOLIFIELD. Of course, we are precluded from making any nuclear shots for detection device recording techniques under our moratorium agreement. If the moratorium agreement is renewed, it would mean that there would be probably no nuclear test explosions at all.

I had a conference with Mr. Tsarapkin on this at Geneva, and I suggested that the United States and the Soviet Union embark on a coordinated program of research and development in which we would. have both chemical shots and nuclear shots. This was 2 years ago. No. It was a year ago last June.

At that time Mr. Tsarapkin stated that they would agree to no more nuclear tests for any purpose, even joint participation tests in which further detection techniques could be verified or ascertained.

Do you have any knowledge of the amount of money that is being spent in concealment experimentations such as Cowboy?

Dr. ROMNEY. No, sir; I have no direct knowledge in that area.

Representative HOLIFIELD. Are there any questions of Dr. Romney or Dr.Latter?

Representative BATES. In respect to what you hope to achieve by $30 million a year, including the nuclear explosions, in the event that the moratorium continues in effect, what will be the impact of that upon what you hope to uncover in the next 3 years?

Dr. ROMNEY. This point will be discussed more in detail later in the program. In general, I think the kind of information you will get without nuclear explosions will be far less specific to the control problems.

You certainly can make progress with chemical explosives rather than nuclear, but you will definitely be sacrificing some things. But this will be discussed in detail.

Representative HOLIFIELD. Dr. Latter, one more question.

You stated that a tenfold improvement in concealment would require a hundredfold improvement in detection. Does this mean that we must make 10 times the effort, or necessarily spend 10 times as much money on the improvement program as we do on concealment?

Dr. LATTER. I do not know whether you can measure this in terms of money. We certainly are faced with a more serious problem in the improvements in detection when faced with increases in concealment.

Representative HOLIFIELD. At least you feel that it is a more difficult scientific problem and more effort should be put into that field?

Dr. LATTER. Yes, sir.

Representative DURHAM. Mr. Chairman, at that point, the world is demanding some kind of agreement. We have been holding a conference for almost 2 years on this problem. The question arises in my mind as to the factual data for reaching some kind of agreement on tests and whether or not we can feel safe under any condition on the basis of research and development work that has been done so far.

I do not care whether you comment on that. We have not finished our research; we have not furnished enough money to do this job.

Representative HOLIFIELD. We have not finished our hearings, Mr. Vice Chairman. Maybe you better withhold your judgment because we will have some people who are pessimistic contrasted to these gentlemen's optimistic position, whichever way you want to put it.

Representative DURHAM. All right; I will withhold my comments.

Representative HOSMER. Whatever you might devise to your own satisfaction would not only be an adequate but a proper type of detection machinery, but there is still the problem of getting that accepted. I would judge from what you have said that you cannot lay down a fact that is uncontrovertible that the other fellow could not nit-pick at it some way so that it would not be accepted. Therefore, your system would be derogated.

Dr. LATTER. I am not sure I understood your point.

Representative HOSMER. I am talking about a negotiation with another country for the sytem that is to go into effect. You would have devised what you believe to be the good system. But could you, with enough definiteness, state why, so that the other fellow could not say "That is not so good here; you make some assumptions there and we are just not going to buy it."

Dr. ROMNEY. Let me try. I think in general you can make fairly good technical arguments as to what kind of equipment you need. There is still room for nit-picking even here, but you can make fairly good and fairly convincing arguments usually.

On the other hand, I think the real problem you get into is what one side or the other considers as adequate for the job. We are interested in detecting the Russians if they should cheat and I agree with Dr. Panofsky it is not clear that they are interested in catching us if we cheat because they know we will not.

Representative HOSMER. Thank you.

Representative HOLIFIELD. Do either one of you have any further statements?

Dr. ROMNEY. No.

Dr. LATTER. No.

Representative HOLIFIELD. Thank you very much for your testimony.

(Dr. Romney's formal statement on "Means for Improving Seismic Detection" follows:)

Means for Improving Seismic Detection

I. BASIC PROBLEMS IN DETECTING UNDERGROUND NUCLEAR EXPLOSIONS

1. Effects of subterranean disturbances at long range

The high energy radiations from nuclear explosions deep underground are rapidly absorbed by the surrounding rock, and most of the energy is converted into heat and compressional elastic waves. At each discontinuity in the physical properties of the rock (including the discontinuity at the surface) part of the compressional elastic energy is converted into other types of elastic waves. The result is that all of the known types of seismic waves (i.e., body waves of compressional and shear types, and surface waves of Rayleigh and Love types) are generally observed to propagate from an explosion. These seismic waves are the only known effects of such explosions which are detectable at distances beyond a few thousand feet from the point of detonation. The amplitude and frequency of the generated waves depend upon such factors as the yield of the explosion, the physical properties of the surrounding medium, the size and shape of the underground shot chamber, the depth of the explosion, the geological conditions and topography of the immediate surrounding area. At great distances, the characteristics of the seismic waves depend additionally on structural and elastic parameters of the earth through which the waves propagate. Little is known about the possible variations in the characteristics of seismic waves from underground explosions since only a limited number have been fired, all of which were in the same material under very similar circumstances.

2. Detection

Seismic waves are detected by measuring the displacement or strain along the surface of the earth. Although the motions are minute (of the order of 10 Angstroms at 1 cycle per second at a distance of 1,000 kilometers from 1 kiloton) instrumental techniques are sufficiently well developed so that these motions can be measured with little trouble. In this part of the spectrum, the limit to detection is thus imposed by natural microseismic noises in the earth. The properties of the microseismic noise are not well known, although it is possible to make general statements about amplitudes in the 0.1 to 10 cycles per second range. However, since the observed noise depends on the intensity of the source, the distance, and the path of propagation, the amplitude is found to be highly variable from place to place and from time to time. At 1 cycle per second, for example, noise amplitudes of the order of 10 Angstroms are observed under extremely quiet conditions, and amplitudes of 200 Angstroms and greater are observed under noisy conditions. More detailed knowledge is required on the spectrum and coherence of noise, particularly at other frequencies, for complete understanding of the detection problem.

3. Identification

A far more serious problem than the detection of seismic waves is the problem of identifying the nature of the source. This is one of the classic problems of seismology, which to date has been solved only under very specialized conditions. The seriousness of this problem is apparent when it is considered that kiloton-size bombs produce waves equivalent to and under most circumstances indistinguishable from very small and very numerous earthquakes. Thus, at the present time, it is possible only to classify seismic disturbances into two categories:

(*a*) Identified earthquakes.
(*b*) Unidentified earthquakes and explosions.

Since the final identification of an underground nuclear explosion requires obtaining a sample of nuclear debris through onsite inspection, the difficulty of such identification is directly proportional to the number of seismic disturbances in the second group. The most significant area of potential improvement is therefore to be found in the development of techniques for identifying the nature of seismic sources.

4. Concealment

A variety of methods are available if special efforts are made to conceal an underground nuclear explosion. These include the use of noise cover from large earthquakes and aftershock sequences which are observed to follow such earthquakes, the placement of the bomb in a position where a given seismic network has a minimum detection capability, and the firing of multiple explosive charges to produce confusing signals. However, on the basis of theoretical analyses, by far the most effective method appears to be that of reducing the size of the distant seismic signal by suitable design of the shot chamber. The effectiveness of the

latter method for decoupling seismic waves has been amply demonstrated, at least for high explosive sources of the order of 1 ton, by the recent experiments in Louisiana (Operation Cowboy).

II. IMPROVEMENT TECHNIQUES

The most complete study of seismic improvement techniques was conducted by the Panel on Seismic Improvement, Dr. Lloyd V. Berkner, Chairman. The results of this study have received wide distribution and do not require discussion in detail. Briefly, however, the panel concluded that the present difficulties in the detection and identification of small seismic events are fundamental in nature and therefore the solution to these problems requires a fundamental attack on a broad range of seismological problems to be followed by a development program for translating research work into a specific detection system. Specific areas of research which can be expected to lead to improvements are discussed below.

1. Additional stations

An immediately available technique for improving the capability of the control system is through the addition of stations to the control network. The Conference of Experts recognized the value of this approach as indicated by the statement in their final report to the effect that "the dependence between these parameters is such that with an increase in the yield of the explosion or the number of control posts the probability of detection and identification increases, and the number of unidentified events suspected of being a nuclear explosion decreases. On the other hand, for the identification of the increased number of unidentified events resulting from a smaller number of control posts it would be necessary to increase the number of onsite inspections or to make greater use of information coming from sources not subordinate to the international control organ or, if necessary, both." Since many of the detection problems result from the fact that signals of interest may be lost in noise, and since the amplitude of seismic waves decreases rather rapidly with distance, an increase in the number of stations means that the average detected signal will be larger compared to noise and therefore more usable for identifying the probable nature of the source. Three specific proposals have been made:

(*a*) The first proposal is to increase the number of control posts of the detection system. In addition to the seismic improvement, this approach has the merit of reducing the threshold of detectability for air shots as well.

(*b*) The second approach is to add a number of simplified manned seismic stations to the network. These first two approaches have the political disadvantage of placing additional men in foreign countries where they will not necessarily be welcome.

(*c*) The third approach involves the addition of extremely simple unmanned seismic stations to the control network and providing means for telemetering the data to the manned control posts. The disadvantages of this approach are the high cost of installation and operation (of the order of $100,000 initial cost per station and a somewhat lesser cost annually for operation), problems associated with the physical security of the station and of the integrity of the data, and perhaps the problem that this approach requires far greater access to foreign territories than do manned stations because of the greatly increased number of stations required (the most authoritative study, by the Berkner Panel, suggests that the stations should be spaced on the order of 100 miles apart). Advantages of this approach are the greatly increased amount of data from closely coupled explosions, and the expectation that it might be possible to use such a fine spacing that even decoupled shots of a few kilotons could be detected.

2. Improved detectors

There is good reason to believe that the response characteristics of seismographs can be improved and be made more adjustable so that signals can be more effectively detected in the presence of noise. Additional work is clearly indicated at high frequencies (about 10 cycles per second) and at very low frequencies (0.1 to 0.01 cycles per second). A second method requiring further research is in the use of very large arrays of seismometers, perhaps with as many as 100 detectors. The effect of an array is to reduce the response to incoherent noise relative to the response to coherent signals. In conjunction with the investigation of arrays, studies should be made of advanced data processing techniques for improving the signal-to-noise ratio by an amount potentially greater than that resulting from the simple additive methods used to date. A third approach, which has been under active investigation for several years, is the development of detectors for operation in

deep wells several thousand feet below the surface of the earth. Since it is expected that much of the troublesome seismic noise is confined to the near-surface region of the earth, such detectors may be able to reveal signals far smaller than can be detected at the surface.

3. Improved analytical methods

When seismic waves are propagated through the earth, they are subjected to great distortion. The result is that the recorded signals are more clearly characteristic of the structural and mechanical properties of the earth rather than of the source. However, as seismology develops to the point where the causes of these distortions are well understood, it will be possible to remove the effect of the earth from the signals and therefore recover information, which is now lost, regarding the nature of the source. Studies of long-period seismic waves are particularly encouraging in this respect. It has recently been possible to determine the faulting mechanism of a number of large earthquakes through surface wave analysis. If this technique is developed to the point where similar analyses can be made on small earthquakes, it will be possible to make significant reductions in the number of unidentified events. Similar analyses can be made of characteristics of the longitudinal waves with the possible result that criteria in addition to the first motion criterion will be developed. A likely result of continued research will be the development of improved means for determining the depth of focus of earthquakes at depths less than the present limit of about 60 kilometers. Such a technique would make a significant reduction in the number of unidentified events by establishing their depths below the limit obtainable by drilling techniques. Spectral analysis of the seismic waves from earthquakes and explosions may reveal significant differences. Experimental measurements of such spectra will, of course, have to be supported by active theoretical investigation of the phenomenology of underground explosions and also of wave generation by faulting sources.

III. EXPECTED RESULTS FROM RESEARCH

A vigorous research program in seismology, at the approximate level recommended by the Berkner Panel (about $30 million per year including $12 million for large nuclear and chemical explosions) can be expected to result in important improvements in the ability to detect and identify earthquakes of small magnitude. Although forecasts of results of scientific research are intrinsically speculative, the Berkner Panel concluded that in 3 years the research program would "probably result in further improvements which could achieve the same capability in the Geneva net of 180 stations as was originally estimated by the Geneva Conference of Experts." This estimate, then, suggests that the control system can be improved to the point where there will remain 20 to 100 unidentified seismic events annually on the continents.

At the same time, there seems to be no reason to doubt that more effective concealment techniques will be developed. It should, therefore, be recognized that the essential nature of the control problem will remain unchanged, i.e., a system consisting of a given number of control stations can be made more effective, but there will always remain a threshold below which nuclear weapons can be tested with little risk of detection. The yield corresponding to the control system's threshold will be reduced in some proportion to the amount of effort applied to the development of detection methods, and increased in some proportion to the amount of effort applied to concealment.

Representative HOLIFIELD. Our next item is means of circumventing detection, commonly termed "decoupling," and we will have Dr. Albert L. Latter of the Rand Corp., of Santa Monica, on this subject.

Dr. Latter is from the University of California and he has been a staff physicist since 1951 at the Rand Corp. in Santa Monica. His special field of interest is the effects of nuclear explosions. (See app. 7, p. 851.)

He was a member of Technical Working Group II of the Conference on the Discontinuance of Nuclear Weapons Tests. I am going to tell a little story here, but it is not one of about these brothers. I do not want any confusion in the minds of the witnesses. There are other

occasions where there are two brothers in science. It seems to run in families.

But the story that I am going to tell does not concern the two Latter brothers. It concerns two other brothers. One of them was the smart one and the other was the dumb one. The smart one got straight A's through college and the dumb one had one B at the end of his college career.

I am not applying that to you, Dr. Latter. I thought it was a good story because I think we are very privileged to have very fine scientists in America and we are always pleased to have gentlemen of your background appear before us. The backgrounds of all the men that have and will appear on this particular set of hearings will be in the record.

You have a statement, I believe, Doctor. You may proceed.

STATEMENT OF DR. ALBERT L. LATTER,[1] RAND CORP., SANTA MONICA, CALIF.

Dr. LATTER. I am sorry that the story you told is not more applicable than it actually is.

Mr. Chairman and members of the committee, to evaluate the capability of the seismic control system, we must know how the strength and character of seismic signals from underground nuclear explosions depend upon all the conditions of the explosion which can be controlled by the people carrying out the test.

These conditions include not only the yield of the explosion but they include also the depth, the medium, the local geology, and the size as well as other characteristics of the explosion chamber. All these factors can be controlled, and we must assume that anyone wishing to violate the agreement would choose that combination of conditions which gives a minimum signal.

Unfortunately, as you have heard, our knowledge of the seismic signals from underground nuclear explosions is quite limited. We have had only a few such explosions and all of these have been in the same medium, the same depth, and the same geology.

Moreover, the size of the shot chamber has in all cases been small compared to the size needed to have a marked influence on thse signal; that is, all these shots have been tamped shots.

This last factor, the size of the shot chamber, is now known to be particularly important and is the basis of the so-called big-hole method of decoupling. I will talk about this method at some length not only because it affects the strength of the signal so drastically, but also because it is the method about which we have the most information at the present time.

To give a quantitative understanding of the big hole requires the use of a lot of mathematics. However, the essential physical idea is a very simple one. If the explosion occurs in a small hole—as is the case for a tamped explosion—the pressure which acts on the surrounding medium is very great and the medium is not strong enough to stand the pressure. As a result, the hole must expand and this causes a large motion of the surrounding earth. It is this motion, this

[1] Received a Ph. D. in theoretical physics from the University of California at Los Angeles in 1951. Since then he has been a staff physicist at the Rand Corp. in Santa Monica, Calif. Dr. Latter's special field of interest is the effects of nuclear explosions. He was a member of Technical Working Group II of the Conference on the Discontinuance of Nuclear Weapon Tests.

large motion, in the immediate neighborhood of the explosion which shows up at great distances as a tiny seismic signal.

On the other hand, if the explosion is made in a big hole, the pressure which the surrounding medium experiences is not very great and the medium can stand the pressure. As a result, there is very little expansion of the hole and essentially no motion of the surrounding earth.

I think you can see from this description that there is not really any question as to whether the big hole works in principle—it does. The only question is: How big does the hole have to be to insure that the pressure from the surrounding medium is not too great?

Before turning to this question of how big the hole must be and if it is big enough what sort of reduction in signal will occur, I want to mention a rather curious fact; namely, that once the hole is big enough, the signal will not be further reduced by making the hole any bigger.

As a matter of historical interest, this fact, that the signal does not depend on the size of the hole, was known to U.S. scientists prior to the first Conference of Experts in 1958.

Representative HOLIFIELD. Doctor, you are skipping part of your statement. We have not had a chance to see it. Would you mind not skipping paragraphs in it? I would like to be educated on this subject.

Dr. LATTER. It was an educational paragraph and I thought perhaps it was not necessary. But if you would like to be educated, very well.

Representative HOLIFIELD. What may not be necessary to you is absolutely essential to me.

Dr. LATTER. To go back to the point as to why the hole need not be made any bigger once it reaches the critical size, this happens because of an accidental cancellation of two opposing physical effects. One of these is that as the hole gets bigger, less energy goes into the earth's motion. This, of course, has a tendency to reduce the signal.

On the other hand, it turns out that the energy which does go into the seismic wave is of a longer wavelength. As the hole gets bigger the wavelength increases. This longer wavelength signal is just the kind of a signal which can be propagated to great distances in the earth easily. The shorter wavelength signals tend to get filtered out by the earth.

The net effect of these two opposite phenomena is that the seismic signal which occurs at a great distance from the scene of the explosion is independent of the size of the hole—provided the hole is big enough in the first place. Historically, this fact that the distant signal does not depend on the size of the hole was known prior to the first Conference of Experts in 1958.

What was not known then was that the validity of this argument depends upon the hole being big enough. As a result the erroneous conclusion was reached that a big hole and a little hole would act in exactly the same way and, therefore, that a big hole would not decouple. It was not until the early part of 1959 that we came to understand the importance of making the hole bigger than some critical size in order to insure that the pressure would not be too great on the surrounding medium.

I would like to turn now to the question how large the seismic signal will be from an explosion in a big hole compared to the signal from the same yield explosion fully tamped in Nevada tuff. In other words, what is the big hole decoupling factor?

The answer to this question is based upon measurements of the earth motion in the neighborhood of one of our underground nuclear explosions, the Rainier explosion, in conjunction with the well understood laws of elasticity. The result is, as you very well know, that the seismic signal is reduced by a factor of about 300 if the hole is made in hard rock, such as granite or salt.

I might say that the analysis which is used to make this estimate has been carefully scrutinized by many scientists for more than a year, and we may be quite confident that the decoupling factor of 300 is solidly founded.

Again, in an educational spirit, I would like to point out that not all of the factor of 300 is due to the action of the hole. Part of the effect is due to the medium in which the hole is made. If the hole is made in a soft medium like Nevada tuff, the signal would be reduced by a factor of about 50 instead of 300.

Representative HOLIFIELD. In other words, a 50-kiloton explosion in the Nevada series in the proper size hole would have registered the same as a 1-kiloton explosion on the seismograph?

Dr. LATTER. Exactly. The effect is not as big for a hole in Nevada tuff as it is for a hole in salt, but it is still very big.

Representative HOLIFIELD. So there is a great variance in recording between different types of earth. Now, can you tell us of several different types of earth that are commonly found in the different parts of the world and what the relation is?

You have given us granite as being six times more effective in transmitting signals as tuff. Are there other types, such as volcanic ash, or sand, or limestone, or other types of material on which you have any information?

Dr. LATTER. I am not really qualified to answer this question. However, it is my belief that the materials, salt, granite, and limestone, are particularly strong as rocklike materials go. While there may be some that are even stronger, I suspect that they are not stronger by a very large factor.

Representative HOLIFIELD. In other words, the softer and looser the formation, the less the signal.

Dr. LATTER. No; the opposite is true.

Representative HOLIFIELD. Is not granite harder than tuff?

Dr. LATTER. Yes, sir.

Representative HOLIFIELD. You say tuff would be 50 and granite 300?

Dr. LATTER. Yes. So the harder the rock in which the hole is made, the more effective it is in reducing the strength of the signal.

Representative HOLIFIELD. I was looking at it the other way. I was thinking the same way, but I was applying it the other way.

Dr. LATTER. The main thing, I guess, is that we agree.

Representative HOLIFIELD. Would you use the blackboard and show the difference between tamped shots and coupled shots?

Dr. LATTER. Yes, sir. I think that the point to keep in mind is that all of our normalization of the strength of the seismic signal and how it is correlated with the yield of the explosion is based upon the

rather specific environment in which the half-dozen nuclear explosions have taken place—that is, the so-called Rainier conditions.

Rainier conditions refer to a depth of burial of the order of 800 feet in the Nevada tuff medium, which is soft in the sense that the speed at which sound waves, very gentle pressure waves, propagate in this medium, is comparatively low. The number, as I remember, for this medium, is of the order of 2 kilometers per second.

What we are talking about is comparing these so-called Rainier signals with signals from holes which are made in a different kind of medium—one in which the sound speed is considerably higher—of the order of 5 kilometers per second, and at a much greater depth of the order of several thousand feet. Here is a hole at several thousand feet. I will write three, but that is not meant too precisely.

Representative HOLIFIELD. It may not be meant precisely, but you are giving somebody a big task to go that deep.

Dr. LATTER. I think it is not such a big task, as we will hear in more detail from Mr. Meade. That is just about the depth at which holes are customarily made in the petroleum industry.

Representative HOLIFIELD. When they shoot at the bottom of the well.

Dr. LATTER. Storage cavities for petroleum products are frequently made at this very great depth. I do not want to say too much about that because Mr. Meade will go into detail.

The Rainier explosion took place in a hole, the shot chamber—now I am remembering sort of off the top of my head and I am not too sure of the number—I think the equivalent radius of this hole was about 3 feet.

Without giving away any military secrets, this is something like what you need in order to house a device. In other words, there is a very intimate connection between the weapon itself and the surrounding earth. If you ask what pressure the surrounding medium is going to see, you can take the total energy which has been released in the explosion and ask what pressure it creates in a hole of that size.

Since that is a very tiny size hole, it is going to be a very large pressure. For decoupling, we are talking about holes which are much larger. For the same yield explosion which we talked about in the case of Rainier, 1.7 kilotons—again I do not remember the figure exactly—the radius of the decoupling hole is of the order of 100 feet. So the 3 feet is to be compared with 100 feet.

Representative HOLIFIELD. Is that 300 feet in diameter?

Dr. LATTER. I said 100 feet in radius, to be compared with 3 feet in the case of the Rainier explosion. Now, because that is such a big volume, the same energy release is going to make a much smaller pressure on the surroundings.

Is this point clear?

Representative HOLIFIELD. It compresses the air in the hole.

Dr. LATTER. The important point is what pressure does the earth feel because it is the earth that is going to generate a seismic wave.

Representative HOLIFIELD. The air acts as a cushion before the shock wave hits the solid earth.

Dr. LATTER. Actually, if there were no air it would be even better because then the energy would first go into the very large volume of the hole and the first time the earth knew anything about the energy the pressure would be much lower.

Representative HOLIFIELD. Pounds per square inch would be much less?

Dr. LATTER. Yes.

Representative HOSMER. Your energy is expended in the vacant space rather than transmitted to the surrounding earth.

Dr. LATTER. Yes.

Representative HOLIFIELD. You told us about a hole being 33 times as large for a 1.7. Let me give you a problem.

If we wanted to explode a 100-kiloton nuclear test in granite, how big a hole would you have to have to reduce the pressure to where the recording would be less than 4.75?

Dr. LATTER. There are two ways to work that problem. One is to be smart. The other is just to remember. If you do not mind, I will choose the second way.

Representative HOLIFIELD. This does not necessarily mean that you are identifying yourself as the brother who got the B?

Dr. LATTER. I hope it does not mean that.

A 100-kiloton explosion to look like 20 kilotons requires a decoupling factor of only five. You have to remember I have been talking about holes which are sufficiently large to produce a decoupling of 300.

The question of how you proceed to make a hole which makes a smaller decoupling factor is a very complicated one and about which we acquired information only recently in the Cowboy experiments. I plan to make some remarks about that particular question, and you might prefere to wait. But if not——

Representative HOLIFIELD. You are not trying to evade the problem?

Dr. LATTER. No. I am trying to give myself enough time to think of the answer. A factor of five is such a small decoupling that it does not come directly from the experiment. What we learned from the experiment was that we could obtain a decoupling factor of about 30 with a hole size about one-thirtieth that which we had specified as a sufficiently large hole. That number, you remember, is 7 times 10 to the fourth cubic meters per kiloton, which we specified in Geneva to Technical Working Group No. II as the volume which would be sufficiently large to give the full decoupling factor of 300.

For a 100-kiloton explosion, this means 7 times 10 to the 6 cubic meters to give a decoupling factor of 300. The Cowboy experiments showed that to get a decoupling factor of 30 we need a volume only one-thirtieth of this size.

One-thirtieth of 7 times 10 to the 6 is 2 times 10 to the 5 cubic meters—for 100-kilotons to look like 3 kilotons. To make 100 kilotons look like 20 kilotons takes an even smaller hole, but I have no direct experimental information on this point.

Representative HOLIFIELD. Interpret that in terms of the diameter of the hole.

Dr. LATTER. All right. That is something I can do with arithmetic.

It is 37 meters or of the order of 120 feet in radius. In diameter, 240 or 250 feet, if I have not made a numerical mistake.

Representative HOLIFIELD. That gives us an idea; and if you want to work that problem out later and correct it for the record you may. If you want to change it you are given that privilege.

Dr. LATTER. Thank you.

Representative HOLIFIELD. Proceed, sir.

Dr. LATTER. I have already recalled to you the figure of 70,000 cubic meters per kiloton of yield for the required volume of the hole as being the safe volume at which we expect to achieve the full decoupling factor of 300.

I have an illustration in my statement of the 20-kiloton explosion requiring for that full decoupling factor a diameter of the order of 140 meters, which I translate into feet as 450, which is a very big hole. It is a big hole compared to what we are used to.

But there is a point I want to make. In my opinion, the only meaningful way to judge the size of a hole is to ask "Can such a hole be made? How much does it cost? How long will it take?"

These are questions which fall outside of my competence, but my understanding from the most expert construction engineers in our country is that such holes are feasible and indeed could be made to accommodate yields all the way up to 100 kilotons and possibly greater at a reasonable cost and in a reasonable time.

Representative HOLIFIELD. We are going to have some testimony from Mr. Meade on that; the next witness.

Dr. LATTER. That is right. I will not say any more about it. I think you will find his numbers are exceedingly interesting.

I want to remind you also that we stipulated such a very large size for the hole in order to be sure that the full decoupling factor of 300 would be achieved. In other words, this criterion was a conservative one and actually one which was laid down prior to any experiments.

It was very important, therefore, to have experiments which would determine how the decoupling factor might be reduced if the hole size were decreased below the safe limit. This was actually one of the principal reasons for the so-called Cowboy tests which were held recently in Louisiana.

I would like to describe those tests in a little detail. They were conducted with conventional explosives in a salt dome medium at a depth of about 800 feet. The mass of the explosive charges used in these experiments ranged from 20 pounds to 2,000 pounds.

Two spherical cavities were used—one having a diameter of 12 feet and the other a diameter of 30 feet. Corresponding to all cavity explosions, there were also fully tamped explosions of the same yield in the same medium and at the same depth.

The seismic signals produced by the tamped explosions could be compared directly with the signals produced by the same yield explosions in the cavities, and this comparison could be made at various distances from the point of the explosion both on the surface and underground.

The experiments were a complete success, and from them we obtained all the information that we had hoped to obtain. The experiments demonstrated beyond any doubt that the big-hole principle of decoupling is correct. Specifically, the experiments showed that the seismic signal from a fully tamped explosion is about 120 times bigger than that from the same yield explosion in the cavities.

This figure of 120 results from the analysis not just of a single piece of data, but of many measurements made for several yields of explosion and at various distances from the point of the explosion.

A value of 120 is a big decoupling factor, but we must ask the question: Why does this value of 120 differ from the decoupling factor of 300 which I previously mentioned? The answer to this question is

that the Cowboy experiment compares the signal from a hole in salt with a signal from a fully tamped explosion in salt, whereas the factor of 300 refers to the signal from a hole in salt compared to a fully tamped explosion in Nevada tuff.

That is our normalization for all of the seismic signal yield correlations. The latter comparison with the Nevada tuff is the one which is relevant for the Geneva detection system. The best estimate of this decoupling factor is still 300. The ratio of 300 to 120—namely 2½—is the amount of decoupling which can be achieved simply by exploding the bomb in salt rather than tuff without making use of a big hole at all.

This is a very important byproduct result which has come from the Cowboy experiments. It says that the seismic signal from an underground explosion can be reduced by a factor of two or three simply by changing the medium, and it raises the question whether other media might give even smaller signals. This question remains to be answered by further experiments.

Representative HOLIFIELD. By the way, what was the size of the chemical shot in the Cowboy series? Did you have different sizes?

Dr. LATTER. Yes. They varied from about 20 pounds to of the order of 2,000 pounds of chemical explosive.

Representative HOLIFIELD. That is TNT?

Dr. LATTER. Yes.

Representative HOLIFIELD. How could that be explained in tons?

Dr. LATTER. 2,000 pounds is a ton. Therefore, about one one-thousandth of the yield of explosion that we normally refer to in the nuclear domain.

Now let me come to the question: What happens to the decoupling factor—and this goes to the earlier question which you raised—if the hole size is made smaller than the critical size of 7 times 10 to the 4 cubic meters per kiloton?

The results of the Cowboy experiments in this connection were very surprising. The best theoretical judgment prior to the experiment was that the hole would behave like a boiler, which when subjected to too large a pressure loses its effectiveness in an abrupt and violent way.

The Cowboy experiments, however, showed that the hole does not behave in this way; rather, it is possible to reduce the hole size by a large factor and at the same time maintain an appreciable amount of decoupling. In one case the hole was too small for the explosion by a factor of about 15.

But the decoupling was degraded only by a factor of four or five. Furthermore, the damage to the cavity was slight and it was possible to use it again. This time the yield of the explosion was doubled. The decoupling was further reduced but only by another factor of two.

These experiments indicate that it would be possible to reduce the size of the hole from the safe volume we have stipulated—namely, 70,000 cubic meters per kiloton—to a size only one-thirtieth as great, and still achieve a very appreciable decoupling factor, perhaps of the order of 30.

Representative BATES. What accounts for that:

Dr. LATTER. Nature, but if you are asking do I understand it thoroughly, the answer is "No." We are now involved in the behavior of materials under pressure conditions in which the simple laws of elasticity do not apply. The laws of elasticity, which are

good laws and have been verified in many circumstances, apply only when the pressures are not very great. When we make the hole small, we subject the medium to a very great pressure and we do not have enough theoretical understanding to say precisely what happens under these conditions at this time.

Representative DURHAM. But it gave you more information than you ever had before.

Dr. LATTER. Yes. That, fortunately, always is the case.

Representative HOLIFIELD. You are probing, you might say, a completely new area of science here.

Dr. LATTER. Yes, sir.

Mr. RAMEY. You came up with a figure on your blackboard calculation of around 250 feet, as I recall, on the diameter of your hole by your decoupling factor of 5 or so?

Dr. LATTER. Was that 5 or 30: I think I was saying in that case you would decouple by 30.

Mr. RAMEY. That would then correspond with the same statement that you have here?

Dr. LATTER. Yes, it is the statement that goes with the Cowboy information.

Representative HOLIFIELD. Will you at your leisure in time for the printed record work out three sizes, 20 kilotons, 50 kilotons, and 100 kilotons?

I am thinking of the sizes of tests which would be necessary to prove small weapon improvements. Work out the formula in salt and also in tuff and submit them for inclusion in the record.

Dr. LATTER. For both the full decoupling and some smaller decoupling?

Representative HOLIFIELD. Yes, sir. So we will have a spread of examples worked out in terms that a layman can understand exactly the size hole that is necessary to decouple these tests both partially and fully.

Dr. LATTER. I will be glad to do that.

Representative HOLIFIELD. I wanted kilotons, patterned after the Rainier shot—not the Rainier shot—but the 19-kiloton shot in Nevada.

Dr. LATTER. I think I understood what you meant.

(The following table was furnished for the record by Dr. Albert Latter:) (See also Dr. Bethis supplementary statement, p. 182.)

Nuclear yield (kilotons)	Diameter of hole to decouple by 300 in salt (or 50 in tuff)	Diameter of hole to decouple by 30 in salt (possibly 5 in tuff)
	Feet	*Feet*
10	360	115
20	450	145
50	610	195
100	770	250
300	1,100	360

NOTE.—The apparent yield as measured on distant seismographs is the nuclear yeild divided by the decoupling factor. E.g., a 100 kilotons explosion in a 250-foot-diameter hole in salt would have an apparent yield of about 3 kilotons.

Dr. LATTER. Yes, sir.

Representative HOLIFIELD. If you need any help, ask your brother.

Dr. LATTER. These results which I have presented are based upon experiments with conventional rather than nuclear explosives. It is proper to ask whether our conclusions would have been different if nuclear explosives had been used.

Here we have only theoretical analysis to rely upon. Theory says that for two explosions of the same yield, one conventional and the other nuclear, in the same size cavity, the conventional explosive creates a much more severe pressure on the cavity wall than the nuclear explosive does.

Representative HOLIFIELD. This is till theory. You have never tested a nuclear device in a large hole.

Dr. LATTER. That is right.

Representative HOLIFIELD. All tests were in tamped holes.

Dr. LATTER. That is right. This conclusion, however, is drawn from theory which is much better established than the theory which I said was not well established before. This theory makes use mainly of hydrodynamic behavior in air at a high pressure level, and that is something which is well understood, in conjunction with the laws of elasticity, which are also well understood, and therefore, we may be much more confident of this theoretical result than we would be of theoretical results for tamped explosions.

To follow your remark, I do believe it is quite important to verify this result with nuclear explosions.

Representative DURHAM. To get complete results and answers to your problem?

Dr. LATTER. Yes; that is right. To have complete results and real certainty you need to test the real thing. By way of summary, let me enumerate what are the important conclusions from the Cowboy tests.

1. The decoupling principle is correct.

2. The decoupling factor for a hole in salt relative to a tamped shot in salt is about 120.

3. The decoupling factor which is relevant for the Geneva system is for a hole in salt relative to a tamped shot in Nevada tuff. The best estimate of this quantity is still 300.

4. The difference between the factor 300 and the factor 120 is explained as due to a medium effect for tamped explosions. In other words, a decoupling factor of two or three results simply by using salt rather than Nevada tuff as the explosion medium.

5. A violator of the test ban would not have to build a cavity with a large margin of safety. The decoupling factor is reduced by exceeding the safe limit but only in a gradual way.

6. For explosions of the order of 10 or 20 kilotons, which is a militarily significant yield, it is not necessary to achieve the full decoupling factor of 300. For such explosions, a decoupling factor of 30 or 40 would be ample to insure that they would not be detected by the proposed Geneva system.

This smaller amount of decoupling can be obtained by using relatively small holes; in fact, a size in which holes are customarily made for storing petroleum products. In other words, it appears possible with existing holes to conceal explosions of the order of 10 or 20 kilotons from the proposed Geneva system.

This last conclusion I think is of particular importance, and it would be desirable to provide further confirmation of this conclusion with explosions of greater yield and of a nuclear rather than a conventional type.

Before concluding, I would like to add a few words about avenues of research that need to be explored in connection with decoupling. We know that the big hole makes a drastic reduction in the seismic signal. We know that a change of medium can make a small but important reduction in the seismic signal.

We believe that changing the depth of the explosion can also influence the signal but we do not have any experiments for this purpose. We need such experiments.

The effect of local geology is also unexplored. We need experiments with explosions in different geologic areas. Finally, I believe there are possibilities for increasing the decoupling, of making further refinements in the big hole technique. For instance, it would be possible to pump out a large portion of the air in the big hole.

Theoretical considerations indicate that this procedure would allow the size of the cavity to be reduced without any reduction in the decoupling factor. Another possibility is to introduce into the cavity some heat-absorbing materials like finely divided carbon, which have the ability to absorb the energy of the explosion without making pressure.

This last technique I think might allow a substantial increase in the decoupling factor without increasing the size of the hole. Some preliminary calculations indicate that a decoupling factor of several thousand might be achieved in this way. In short, I believe there are still many advances to be made in the art of concealing underground nuclear explosions.

I also believe that methods of concealment must be studied simultaneously with methods for improving detection, so that at all times we can make the best evaluation of the capabilities of the control system.

Thank you.

Representative HOLIFIELD. Thank you very much, Dr. Latter. We will expect to have these problems from you. (See table, p. 131.)

Dr. LATTER. I will do my best. I hope I get at least a B.

Representative HOLIFIELD. Mr. Bates?

Representative BATES. What would be the effect if, instead of on the surface, that you dug this hole and created an explosion in a mountain where you have horizontal pressure going into the atmosphere? What effect would that have?

Dr. LATTER. I do not fully understand you. Are you allowing the products of the explosion to vent into the air?

Representative BATES. Take a mountain peak. If we dug down through there you would have certain horizontal pressures that will go into the air, and you will have others that will only be downward as far as the seismic readings are concerned.

Dr. LATTER. I think that if the hole were in such a protrusion, but the dimensions of that protrusion were very large compared to the size of the hole—remember, we are talking of something of the order of a few hundred feet for the diameter—if the lateral dimension were large compared to that, I think offhand that it would be equally effective in decoupling the signal and maybe more effective.

Representative BATES. There might be some volcanic craters that could be used.

Dr. LATTER. You must be careful that the depth of the hole is sufficiently great. Part of the strength of the medium comes from having a lot of rock weighing on top of it.

A rocklike material has very little strength when it is in tension, but as much strength as other materials as long as it is in compression. So the idea is to get a lot of rock weighing on top of it to keep it in compression, which provides the strength.

In other words, if you make an explosion in a hole in a mountain, you must still be careful to keep the depth of the hole sufficiently great relative to the top of the mountain.

Representative BATES. I just wondered because some of the action is on the horizontal effect, whether that would nullify the effect of the decoupling to a certain degree.

Dr. LATTER. I think if the dimensions were sufficiently large in the lateral direction compared to the size of the hole, it would not nullify the effect. But if they were not sufficiently large it would nullify the effect, and the explosion might not be contained and it would just break through the mountainside.

Representative HOLIFIELD. Is it your opinion that the hole should be circular?

Dr. LATTER. It is important that it not differ too much from sphericity. By "too much" I mean a factor of two or three. What is not all right is making it a long, cigar-shaped thing.

Representative HOLIFIELD. Most of the natural gas cavities are in the long shape, as I understand it.

Dr. LATTER. Mr. Meade will explain to you that they are in the shape they are in because people want them in that shape and it is just as easy to make them in a nearly spherical shape.

Representative DURHAM. I think one of the most important statements you make is that you can carry this decoupling factor on down and down and it winds up that this kind of detection is not going to be worth very much.

Dr. LATTER. It is a good idea to take into account the possibility that if extraordinary means of this sort are made use of that the signal will be so reduced that the detection system will have no capability whatsoever against it.

Representative DURHAM. It is possible, in your opinion?

Dr. LATTER. In my opinion, at some yield.

Representative HOLIFIELD. Thank you very much, Doctor.

Dr. LATTER. Thank you.

(Dr. Albert L. Latter's prepared statement follows:)

DECOUPLING OF UNDERGROUND NUCLEAR EXPLOSIONS

(By Albert L. Latter, Rand Corp., Santa Monica, Calif.)

To evaluate the capability of a control system for monitoring an underground nuclear test ban, we must have a great deal of information about the seismic signals which are produced by underground nuclear explosions. In particular we must know how the strength and the character of the signal depend upon all the conditions of the explosion which can be readily controlled by the people who are carrying out the test. These conditions include not only the yield of the explosion, but also the depth of the explosion, the medium in which it occurs, the local geology of the region, and the size as well as other characteristics of the explosion chamber. All these factors can be controlled, and we must assume that

anyone wishing to violate the agreement would choose that combination of conditions which gives a minimum signal.

Unfortunately our knowledge of the seismic signals produced by underground nuclear explosions is quite limited. We have had only a very few underground nuclear explosions, and all of these have occurred in the same medium, Nevada tuff. All of them have been at a rather shallow depth of burial, less than a thousand feet. The local geology has been the same for all of them. And finally, the size of the shot chamber has in all cases been small compared to the size which would be needed in order to have a marked influence on the signal, i.e., all of the shots have been more or less closely tamped.

This last factor, the size of the shot chamber, is now known to be particularly important and is the basis of the so-called big hole method of decoupling. I will talk about this method at some length not only because it affects the strength of the seismic signal so drastically but also because it is the method about which we have the most information at the present time.

To give a quantitative understanding of how the big hole reduces the seismic signal, requires the use of a lot of mathematics. The essential physical idea, however, is a very simple one. If the explosion occurs in a small hole—as is the case for a tamped explosion—the pressure which acts on the surrounding medium is very large and the medium is not strong enough to contain the pressure. The hole must expand and this causes large motions in the surrounding earth. If the explosion occurs in a big hole however, the pressure which acts on the surrounding medium is not large, and the medium is strong enough to contain the pressure. In this case there is scarcely any expansion of the hole, and very little motion of the earth. I think you can see from this description that there is not really any question as to whether the big hole works in principle—it does. The only question is: how big does the hole have to be to insure that the pressure from the explosion is not too great?

Before turning to this question of how big the hole must be, and if it is big enough, what sort of reduction in signal will occur, I want to mention a rather curious fact: namely, that once the hole is big enough, the signal will not be further reduced by making the hole any bigger. This happens because of an accidental cancellation of two opposing effects. One is, that as the hole gets bigger, less energy goes into the earth's motion, which tends to decrease the signal. On the other hand it turns out that the energy which does go into the signal has a longer wavelength as the hole gets bigger, and this longer wavelength signal is more easily propagated to great distances through the earth, which tends to increase the signal. The net effect is that the seismic signal which occurs at a great distance from the scene of the explosion is independent of the size of the hole—provided the hole is big enough. As a matter of historical interest, this fact that the distant signal does not depend upon the hole size was known to U.S. scientists prior to the first Conference of Experts in 1958, but at that time they did not realize that this is true only provided the hole is big enough and as a result they came to the erroneous conclusion that a big hole is no different from a little hole and hence a big hole will not decouple the signal. It was not until the early part of 1959 that we came to understand the importance of making the hole bigger than some critical size to insure that the pressure would not be too great on the surrounding medium.

I turn now to the question how large is the seismic signal from an explosion in a big hole compared to the signal from the same yield explosion fully tamped in Nevada tuff: In other words, what is the big hole decoupling factor?

The answer to this question is based upon measurements which were made of the earth motion in the neighborhood of one of our underground nuclear explosions in conjunction with some theoretical analysis which employs the well-understood laws of elasticity. The result is that the seismic signal will be reduced by a factor of about 300 if the hole is in a hard rock medium such as salt or granite.

The analysis which is used to make this estimate has been carefully scrutinized by many scientists for more than a year, and we may be confident that the decoupling factor of 300 is solidly founded.

It may be of interest to note that not all of the factor 300 comes from the action of the hole. Part of the effect is due to the medium in which the hole is made. If the hole were made in a soft medium like Nevada tuff, the signal would be reduced by a factor of about 50 instead of 300. The further factor of 6 comes from making the hole in salt or granite, which are stronger media.

The difficulty imposed on the control system by such large decoupling factors is of course obvious. To use a by-now familiar example, a 100-kiloton explosion in a big hole looks seismically like an explosion of only one-third of a kiloton

under ordinary tamped conditions. Such a weak seismic signal would not even be noticed by the proposed Geneva control system against the natural background of seismic noise.

Turning now to the crucial question of how big the hole must be to produce such a large decoupling, I would like to recall to you the figure that we presented in Geneva to the Technical Working Group No. II, 70,000 cubic meters per kiloton of yield for roughly spherical holes at a depth of about 1 kilometer. To be a little more graphic, this means that for a 20-kiloton explosion the hole must have a diameter of 140 meters. Still more graphic perhaps—450 feet. This is a very large hole compared to holes that we are used to. But, the only meaningful way to judge the size of a hole in the present context is to ask can such a hole be made? how much does it cost? and how long will it take? These are questions which fall outside of my competence, but I have understood that the most expert construction engineers in our country say that such holes are feasible and indeed could be made to accommodate yields all the way up to 100 kilotons, and possibly greater, at a reasonable cost and in a reasonable time. Mr. Meade who is one of these experts will discuss more facts about this important question in a subsequent presentation.

Also I want to remind you that such a large size for the hole was stipulated in order to be absolutely sure that the full decoupling factor of 300 would be achieved. In other words this criterion for the hole size was a conservative one which was laid down prior to any experiments. Experiments were needed to determine how the decoupling factor might be reduced if the size of the hole were made smaller than the safe size. This was one of the principal reasons for the so-called Cowboy series of tests held recently in Louisiana.

The Cowboy tests were conducted with conventional chemical explosives in a salt dome medium at a depth of about 800 feet. The mass of the explosive charges used in these experiments ranged from 20 pounds to 2,000 pounds. Two spherical cavities, one having a diameter of 12 feet, the other a diameter of 30 feet, were available. Corresponding to all cavity shots there were also fully tamped explosions of the same yield in the same medium and at the same depth. The seismic signal produced by the tamped explosions could be compared directly with the signals produced by the same yield explosions in the cavities, and this comparison could be made at various distances from the point of the explosion both on the surface and underground.

The experiments were a complete success, and from them we obtained all the information that we had hoped to obtain. The experiments demonstrated beyond any doubt that the big hole principal of decoupling is correct. Specifically the experiments showed that the seismic signal from a fully tamped explosion in the salt was about 120 times bigger than that from the same yield explosion in one of the cavities. This figure of 120 results from the analysis not just of a single piece of data but of many measurements made for several yields of explosion and at various distances from the point of the explosion.

We must now ask the question: Why does this value of 120 differ from the decoupling factor of 300 previously mentioned? The answer to this question is that the Cowboy experiment compares the signal from a hole in salt with the signal from a fully tamped explosion in salt, whereas the factor of 300 refers to the signal from a hole in salt compared to a fully tamped explosion in Nevada tuff. The latter comparison is the one which is relevant for the Geneva detection system. The best estimate of this decoupling factor is still 300. The ratio of 300 to 120, namely 2½, is interpreted as the amount of decoupling which can be achieved simply by exploding the bomb in salt rather than tuff without making use of a big hole at all. This is a very important byproduct result which has come from the Cowboy experiments. It says that the seismic signal from an underground explosion can be reduced by a factor of 2 or 3 simply by changing the medium, and it raises the question whether other media might give even smaller signals. This question remains to be answered by further experiments.

Now let me come to the question: What happens to the decoupling factor if the hole size is made smaller than the critical size? The results of the Cowboy experiments in this connection were very surprising. The best theoretical judgment prior to the experiment was that the hole would behave exactly like a boiler, which when subjected to too large a pressure loses its effectiveness in an abrupt and violent way. The Cowboy experiments, however, showed that the hole does not behave in this way. Rather it is possible to reduce the hole size by a large factor and at the same time maintain an appreciable amount of decoupling. In one case, the hole was too small for the yield of the explosion by a factor of 15;

but the decoupling was degraded only by a factor of 4 or 5. Furthermore the damage to the cavity was slight and it was possible to use it again. This time the yield of the explosion was doubled. The decoupling was further reduced but only by another factor of two. These experiments indicate that it would be possible to reduce the size of the hole from the safe volume we have stipulated, namely 70,000 cubic meters per kiloton, to a size only one-thirtieth as great, and still achieve a very appreciable decoupling factor, perhaps of the order of 30.

Of course, all these results are based upon experiments with conventional rather than nuclear explosives. It is proper to ask whether our conclusions would have been different if nuclear explosives had been used. Here we have only theoretical analysis to rely upon. Theory says that for two explosions of the same yield, one conventional and the other nuclear, in the same size cavity, the conventional explosive creates a much more severe pressure on the cavity wall than the nuclear explosive does. That is, theory says that the decoupling factor which would be obtained with a nuclear explosive is, if anything, greater than the decoupling factor obtained with a conventional explosive. I think we can be quite confident of this result but I also believe it is important to demonstrate it experimentally with nuclear explosions.

By way of summary let me now enumerate what are the important conclusions from the Cowboy tests.

1. The decoupling principle is correct.
2. The decoupling factor for a hole in salt relative to a tamped shot in salt is about 120.
3. The decoupling factor which is relevant for the Geneva system is for a hole in salt relative to a tamped shot in Nevada tuff. The best estimate of this quantity is still 300.
4. The difference between the factor 300 and the factor 120 is explained as due to a medium effect for tamped explosions. In other words a decoupling factor of 2 or 3 results simply by using salt rather than Nevada tuff as the explosion medium.
5. A violator of the test ban would not have to build a cavity with a large margin of safety. The decoupling factor is reduced by exceeding the safe limit but only in a gradual way.
6. For explosions of the order of 10 or 20 kilotons, which is a militarily significant yield, it is not necessary to achieve the full decoupling factor of 300. For such explosions a decoupling factor of 30 or 40 would be ample to insure that they would not be detected by the proposed Geneva system. This smaller amount of decoupling can be obtained by using relatively small holes, in fact a size in which holes are customarily made for storing petroleum products. In other words it appears possible with existing holes to conceal explosions of the order of 10 or 20 kilotons from the proposed Geneva system.

This last conclusion, I think, is of particular importance, and it would be desirable to provide further confirmation of this conclusion with explosions of greater yield and of a nuclear rather than a conventional type.

Before I conclude I would like to say a few words about other avenues of research that need to be explored in connection with decoupling. We know that the big hole makes a drastic reduction in the seismic signal. We know that a change of medium can make a small but important reduction in the seismic signal. We believe that changing the depth of the explosion can also influence the signal but we do not have any experiments for this purpose. We need such experiments. The effect of local geology is also unexplored. We need experiments with explosions in different geologic areas. Finally, I believe there are possibilities for increasing the decoupling factor by making further refinements in the big hole technique. For instance, it would be possible to pump out a large portion of the air in the big hole. Theoretical considerations indicate that this procedure would allow the size of the cavity to be reduced without any reduction in the size of the decoupling factor. Another possibility is to introduce into the cavity some heat absorbing materials like finely divided carbon, which have the ability to absorb the energy of the explosion without making pressure. This last technique I think offers a realistic possibility of substantial increases in the decoupling factor without increasing the size of the hole. Some preliminary calculations indicate that a decoupling factor of several thousand might be achieved in this way. In short I believe there are still many advances to be made in the art of concealing underground nuclear explosions. I also believe that methods of concealment must be studied simultaneously with methods for improving detection, so that at all times we can make the best evaluation of the capabilities of the control system.

Representative HOLIFIELD. Our next witness will be Mr. L. P. Meade, of the Phillips Petroleum Co., who will testify on the feasibility of the construction of large cavities.

I will place Mr. Meade's biography in the record at this time. Suffice it to say it is a very distinguished one and he has had many years of experience in geological work.

I would like to say it is a pleasure to have you with us today. The committee has always tried whenever possible to call witnesses from those segments of industry not engaged solely in pure research and development who, through practical experience, can also contribute to our search for information.

Mr. Meade, I think we better start after lunch. The bells have just rung for a rollcall in the House. I regret we will have to leave now to answer the rollcall, but we will start at 2:30.

Following Mr. Meade we will have Dr. Teller and Dr. Hans Bethé for witnesses.

(Whereupon, at 12:15 p.m., the subcommittee recessed, to reconvene at 2:30 p.m. the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order. We will continue this afternoon with testimony from Mr. L. P. Meade of the Phillips Petroleum Co. on the feasibility of construction of large cavities.

Mr. Meade, will you please come forward to the chair. I am sorry we had to interrupt you this morning. You may proceed.

STATEMENT OF L. P. MEADE,[1] PHILLIPS PETROLEUM CO.

Mr. MEADE. Thank you, Mr. Chairman.

Mr. Chairman and members of the committee, the principal purpose of my discussion is to provide information on the feasibility of constructing large underground cavities. Such cavities might be constructed in various formations by well-known conventional hard-rock-mining methods. However, such a cavity would be very costly and would take quite a few years to build. It is also believed that many formidable difficulties would be experienced in such an undertaking. A large cavity might also be constructed in a salt formation by solution mining or leaching methods. While these techniques may not be as well known, they are simple and economical. A cavity constructed by these methods is estimated to cost only about one-tenth as much as one mined in hard rock. I shall therefore restrict my remarks to the creation of large cavities in salt by solution-mining techniques.

About 250 underground cavities have been constructed in salt in the United States for the storage of petroleum products—mostly liquefied petroleum gas. A list of these cavities is given in the September 28, 1959, issue of the Oil and Gas Journal. Many more cavities have been formed by salt and chemical companies for the

[1] Manager of the Engineering Development Branch of the Engineering Department of Phillips Petroleum Co. of Bartlesville, Okla. He has been in this position, engaged in consulting work and the direction of special services and engineering studies, for the past 8 years. Prior to this, he was mechanical design supervisor in the Phillips Engineering Department Design Division for a period of 8 years. He became associated with Phillips in 1936 after having spent a year and a half with John Deere Tractor Co. of Waterloo, Iowa. He received his B.S. and M.S. in mechanical engineering from the University of Iowa in 1931 and 1934 and in addition devoted 2 years to research work at the university. Mr. Meade is a registered professional engineer in the State of Oklahoma, a member of the Oklahoma Society of Professional Engineers, The National Society of Professional Engineers, and the American Society of Mechanical Engineers.

production of brine. For the most part, underground cavities have been formed by the solution-mining method, because of much lower cost and similicity of this method over conventional mining.

There are two types of salt formations in which cavities have been formed. These are sedimentary beds on the one hand and salt domes on the other hand. Salt domes are to be preferred because they are purer, more homogeneous, and thicker than sedimentary beds. There are more than 200 salt domes in the gulf coast region of the United States from the Mexican border to western Alabama. These domes range in size from one-half to 7 miles in diameter (0.8 to 11 kilometers) with an average of from 1 to 2 miles (1.6 to 3.2 kilometers). The average depth to the top of the salt is from 500 to 1,000 feet (150 to 300 meters). The average thickness of the salt is over 10,000 feet (3 kilometers) and in many cases is known to be as deep as 30,000 feet (9 kilometers). A map and listing of these salt domes is given in the August 11, 1952, issue of the Oil and Gas Journal. It is a well-known fact that similar salt domes exist in other parts of the world. A survey of the literature reveals references to a large number of areas in the Soviet Union where salt domes exist.

Representative HOLIFIELD. Are these salt domes solid or do they have natural cavities in them?

Mr. MEADE. No, they are solid. To my knowledge, no natural cavities exist in salt domes.

Representative VAN ZANDT. Mr. Meade, give us a definition for solution mining method.

Mr. MEADE. That will follow a little later. I have a complete description of it, sir.

Representative VAN ZANDT. Very well. Thank you.

Mr. MEADE. The cavities which have been constructed for petroleum storage range in size from 25,000 barrels or 140,000 cubic feet (4,000 cubic meters) to 1 million barrels or 5,600,000 cubic feet (160,000 cubic meters). Much larger cavities have been created by chemical companies for the production of salt. One such cavity in the Bryan Mound near Freeport, Tex., is located between the depths of 3,000 and 3,500 feet (915 and 1,065 meters) below the surface and has a maximum diameter of 500 feet (150 meters).

Phillips Petroleum Co. has constructed some 20 cavities in salt, 3 of which are in the Clemons Dome near Freeport, Tex. A fourth is now under construction in this dome. The largest is located between the depths of 1,910 and 2,260 feet (58 and 690 meters) below the surface, is roughly cylindrical in shape, with a maximum diameter of 235 feet (73 meters). It has a volume of 1 million barrels or 5,600,000 cubic feet (160,000 cubic meters). The deepest cavity is located between the depths of 2,840 and 3,110 feet (865 and 950 meters) below the surface, is also roughly cylindrical in shape, with a maximum diameter of 150 feet (46 meters). It has a volume of 250,000 barrels or 1,140,000 cubic feet (40,000 cubic meters). These cavities were purposely made the shape and size indicated. It is easy to make them of other shapes and sizes by use of the same methods of construction. The size can be increased by leaching over a long period of time or at a higher rate or both. The shape can be controlled by a technique involving the use of a hydrocarbon blanket for roof control and the point at which the leaching water is injected and the direction of circulation—whether from the top down or the bottom up. Repeated sonic surveys would be made as construction

progressed to make sure that the cavity would be the actual shape desired.

In order to give you some idea as to the cost and time required for constructing such a cavity let us assume one of 13 million barrels or 73 million cubic feet (2.1×10^6 cubic meters) volume located 2,500 feet (760 meters) below the surface in a salt dome. This cavity would be about 516 feet (157 meters) in diameter if it were spherical. On the basis of criteria given, this size cavity would decouple the seismic signal of a 30-kiloton explosion. Cost of constructing and time for construction are closely interrelated. For this reason I will consider two cases, first that cost of construction is of considerable importance and is to be minimized, and second that time of construction is of paramount importance and is to be minimized.

Representative HOLIFIELD. From your statement so far, sir, you state that there are a considerable number of these large holes in existence at the present time.

Mr. MEADE. There are a considerable number of cavities of 1 million barrels and less.

Representative HOLIFIELD. Speaking in terms of the diameter of the hole, what would this run from? What are the sizes of these holes that are in existence at the present time?

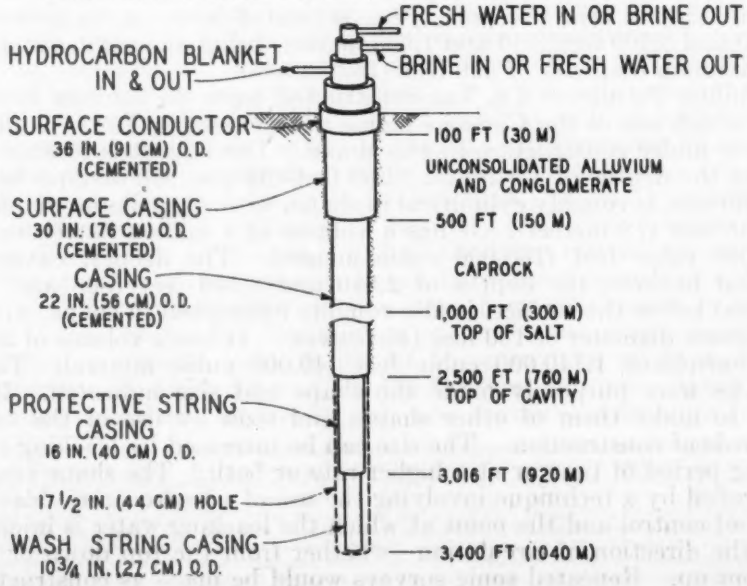
Mr. MEADE. Most of the holes are under 300 feet in diameter that are used for hydrocarbon storage. There are some considerably larger holes that have been created for brine production and some of those run as high as 500 feet in diameter.

Representative HOLIFIELD. Do you go into costs of construction later on?

Mr. MEADE. Yes, sir.

May I have the slide of figure 1, please.

FIGURE 1
TYPICAL SOLUTION MINING WELL


FRESH WATER IN OR BRINE OUT
HYDROCARBON BLANKET IN & OUT
BRINE IN OR FRESH WATER OUT
SURFACE CONDUCTOR
36 IN. (91 CM) O.D. (CEMENTED)
100 FT (30 M)
UNCONSOLIDATED ALLUVIUM AND CONGLOMERATE
SURFACE CASING
30 IN. (76 CM) O.D. (CEMENTED)
500 FT (150 M)
CAPROCK
CASING
22 IN. (56 CM) O.D. (CEMENTED)
1,000 FT (300 M) TOP OF SALT
2,500 FT (760 M) TOP OF CAVITY
PROTECTIVE STRING CASING
16 IN. (40 CM) O.D.
3,016 FT (920 M)
17 1/2 IN. (44 CM) HOLE
WASH STRING CASING
10 3/4 IN. (27 CM) O.D.
3,400 FT (1040 M)

For the first case, that of minimum cost, a single hole would be drilled and cased as shown by figure 1. A surface conductor 36 inches (91 centimeters) in diameter would first be set to a depth of about 100 feet (30 meters) and cemented to the surface. A hole would then be drilled into the caprock and a 30-inch (76 centimeters) surface casing would be cemented to the surface, about 500 feet down. Conventional oilfield cementing techniques would be employed. This casing is necessary to protect the water-bearing formations and to assure success in drilling through the caprock and on into the salt. The caprock is often porous making it impossible to maintain circulation of drilling mud which is necessary for the stabilization of the hole in unconsolidated formations above. The hole would then be drilled on into the salt dome to the desired depth of 2,500 feet (760 meters) to the top of the cavity and a 22-inch (56 centimeters) casing cemented to the surface. The hole would then be drilled to its total depth of 3,400 feet (1,040 meters). A 16-inch (40 centimeters) diameter "protective" casing would be suspended to a depth of approximately 3,016 feet (920 meters) and a 10¾-inch (27 centimeters) wash string suspended to a depth of 3,400 feet (1,040 meters).

That is the type of casing schedule which we usually employ in any cavity construction.

Representative HOLIFIELD. Where is your circulation obtained? Is it up around the protective string?

Mr. MEADE. Yes.

Representative HOLIFIELD. So you remove the salt just as you do the oil.

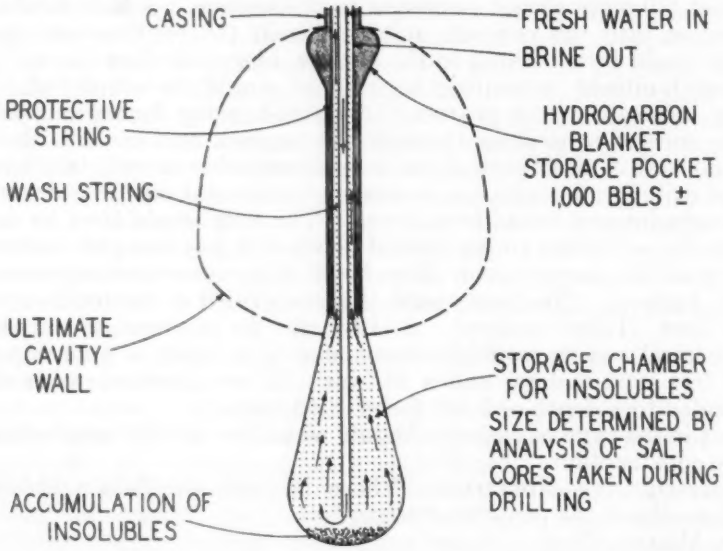
Mr. MEADE. There are a few tricks to this thing to keep the roof from dissolving away and to control the shape and to produce the volume as rapidly as possible.

The next slide, figure 2. Here is the first stage. That is washing the Bell Hole for insoluables. Most of these salt domes contain about 10 percent of insolubles. I am not sure of the chemical formula. The washing operation would be started with the creation of a small space for the hydrocarbon blanket used for roof shape control. See figure 2. This would be done by pumping fresh water down the casing annulus and up the wash string.

I might say that the brine would come on down and then would be brought up the protective annulus. After this space is formed a light oil such as burning oil or kerosene would be injected into the casing, as shown by red on the slide, until we got to this point, at which time we would get returns going up this annulus which I am calling the protective string annulus. Circulation would then be established with fresh water going down the wash string and brine returned up through the protective string annulus. This would be continued long enough to create a bell hole of sufficient size to contain the 10 percent or so of insoluble anhydrite that would be released from the salt during the entire cavity washing operation. As it is washed, the anhydrite which is usually in the form of sand would fall to the bottom and fill the cavity. This is done in order to get a spherical cavity because that is the shape that is wanted.

Representative HOLIFIELD. You use this process at the present time for the storage of oil and gas?

FIGURE 2
WASHING BELL HOLE FOR INSOLUBLES



Mr. MEADE. That is right. Primarily the storage of liquefied petroleum gas, which has to be maintained under a pressure of approximately 200 pounds normally. This is an economical method of storage.

May I have the next slide, figure 3, please. As soon as the anhydrite space has been formed, the wash string and protective strings would be pulled up here to a point just above the top of the cavity and the actual shape determined by a sonic survey. This is in common use.

The actual shape and size would be determined just to check on our progress to date. The wash string would then be reset approximately 500 feet into the hole from the top of the cavity and the protective string down about 100 feet from the top of the cavity. Then the hydrocarbon blanket would be spotted some place near the top and washing would be continued.

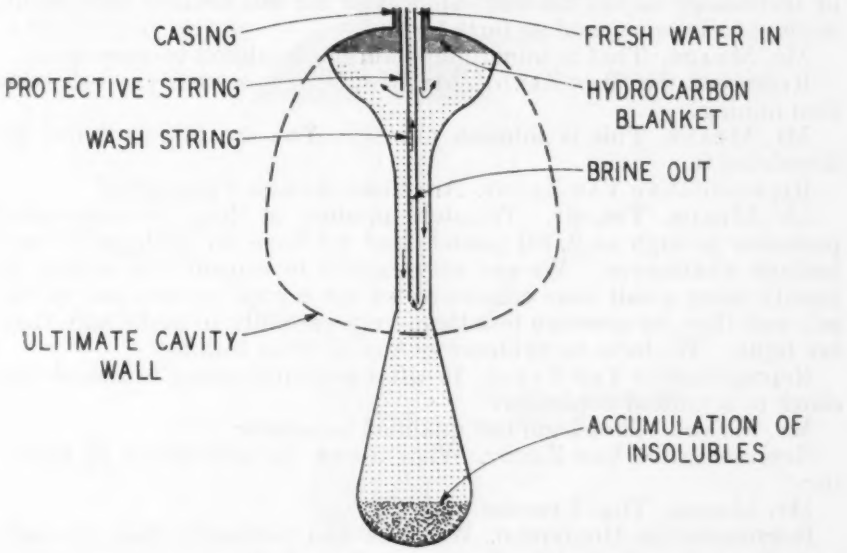
Representative HOLIFIELD. How close would this large hole be to the surface under this situation?

Mr. MEADE. We have assumed a depth of 2,500 feet.

Representative HOLIFIELD. I was going to ask you a minute ago if it is necessary to go that deep, or do you go as deep as your salt formations are?

Mr. MEADE. No, sir. One of the premises on which the criteria is based is that the pressure generated by the explosion will not be greater than one-half a pound per foot of overburden so we don't raise the surface of the earth. I believe that is what determines the depth. We have purposely established this between 2,500 and 3,000 feet. It could be perhaps 3,000 to 3,500 feet. However, we would not want to go much deeper than that due to the nature of the salt which becomes hydrostatic and flows under pressure at depths greater

FIGURE 3
WASHING OF HYDROCARBON BLANKET SPACE



than 3,500 or 4,000 feet and it is not known how much flow will take place. We do know we have cavities at this depth which are successful. How much deeper we can go is not known at this time.

Representative HOLIFIELD. How many of these cavities would you estimate exist in the United States at this time?

Mr. MEADE. I would guess somewhere around 300. I cited 250 for hydrocarbon storage and guessing offhand maybe 50 or more for brine production. I am sure I am on the low side.

Representative HOLIFIELD. The smallest diameter would be how much?

Mr. MEADE. Fifty to seventy-five feet.

Representative HOLIFIELD. The largest?

Mr. MEADE. The largest that I have record of here is about 500 feet in diameter.

Representative VAN ZANDT. Are they distributed throughout the Nation or are they in one section?

Mr. MEADE. For the most part they are in one section, in the southern part of Texas and Louisiana. However, they are also in the salt plains of west Texas, the Texas Panhandle, Kansas, New York State, and Ontario.

Representative HOLIFIELD. Does northern Pennsylvania have any?

Mr. MEADE. Not to my knowledge. I think the salt is rather deep there.

Representative HOLIFIELD. Do you have any knowledge of the Russians using this same process for storage of hydrocarbon?

Mr. MEADE. I have no knowledge of it, sir. It has been widely publicized.

Representative HOLIFIELD. It is public information?

Mr. MEADE. It is all public information. There has been a lot written on it. It would be possible to do it any place there is salt.

Representative HOLIFIELD. How do they rate from the standpoint of technology in the field of exploration for oil, drilling equipment, drilling techniques, and so forth?

Mr. MEADE. That is something I am not qualified to answer.

Representative VAN ZANDT. Mr. Meade, is this what you call solution mining?

Mr. MEADE. This is solution mining. You are taking it out by dissolving it.

Representative VAN ZANDT. Are these cavities watertight?

Mr. MEADE. Yes, sir. We store product in these cavities under pressures as high as 2,000 pounds and we have no evidence of any leakage whatsoever. We are very careful to cement our casings in tightly using a salt base cement so we get a good cement job to the salt and then we pressure test them very carefully to make sure they are tight. We have no evidence of any of them leaking.

Representative VAN ZANDT. Is salt a good ingredient to absorb the shock of a nuclear explosion?

Mr. MEADE. That I am not qualified to answer.

Representative VAN ZANDT. What about the absorption of radiation?

Mr. MEADE. That I cannot answer.

Representative HOLIFIELD. We have had testimony that the salt is similar to granite in its seismic recording effects.

You may proceed.

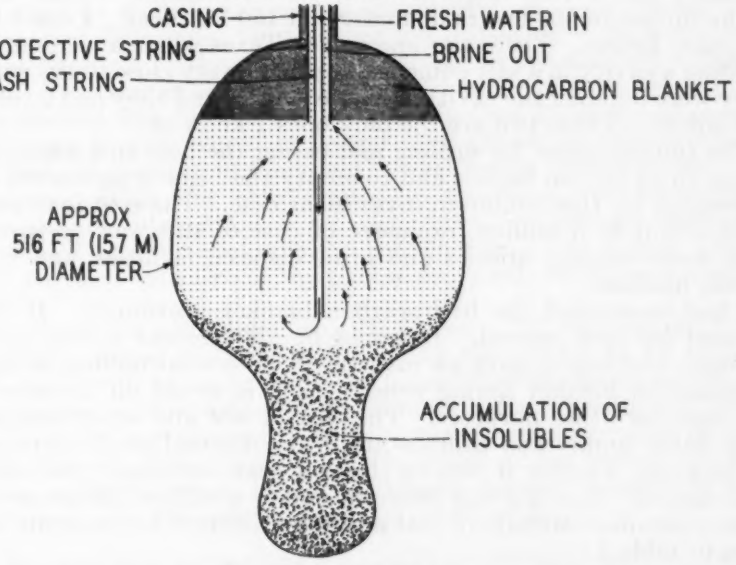
Mr. MEADE. Since the fresh water is lighter than brine, it will tend to float on out and wash this upper portion of the cavity and we will first form this route to be able to control the shape properly. After the top portion of the cavity is formed, the wash string would be pulled and a sonic survey run again in order to check on the shape and size.

May I have the next slide, figure 4, please. The wash string would be reinstalled and the circulation would be reversed. We now would come down the wash string with our fresh water and back up through the annulus of the protective string. This is done in order to remove the salt at a fairly uniform and rapid rate throughout the height of the cavity. When we come in at the top we tend to spread out and make the thing large in diameter at the point of injection. If we reverse our circulation we get a mixing of this brine and this solution over the entire height. We would expect a cavity of about this shape from that washing procedure.

Hydrocarbon would be added from time to time in order to prevent further dissolution and retain the roof shape. Sonic surveys would be run to check on this shape. With the wash strings shown for this case, a maximum circulation rate of approximately 5,000 gallons per minute (315 liters per second) of wash water could be achieved which would require about 4,200 horsepower. It is estimated that 33 months would be required to drill and case the well and wash the cavern to 13 million barrels or 73 million cubic feet (2.1 by 10.6 cubic meters).

Following completion of washing, it would be necessary to empty the cavity of brine. This would be done by removing the hydrocarbon blanket by displacing it with brine and then pulling the 16-

FIGURE 4
WASHING OF CAVITY



inch (41 cubic meters) and 10¾-inch (27 cubic meters) casing and installing a submerged motor electric pump through the 22-inch (56 cubic meters) casing. This pump would have a 1,200-horsepower motor and be suspended on a 10¾-inch (27 cubic meters) tubing through which it would deliver 900 gallons per minute to the surface. At this rate it would take 14 months to empty the cavity.

Representative HOLIFIELD. You have a total then of 47 months.

Mr. MEADE. Forty-eight months, I believe it is.

Representative HOLIFIELD. Which is 4 years, to construct a hole of this type.

Mr. MEADE. That is right.

Representative HOLIFIELD. I see you get to the cost a little later in your statement.

May I pin that amount to the size that is involved in this? What is the diameter of this hole that you are using these figures on?

Mr. MEADE. This was a 516-foot diameter, as I remember.

Representative HOLIFIELD. 516 feet.

Mr. MEADE. Yes.

May I have the next slide, please.

Representative VAN ZANDT. Mr. Chairman, may I ask Mr. Meade about the collapse of these caverns. In my State of Pennsylvania we constantly are faced with cave-in of mines. Would these cavities collapse or are they so constructed that they would maintain their shape?

Mr. MEADE. As I mentioned earlier, there are two types of salt formations. The sedimentary formations which are imbedded with shale and the salt domes which are homogeneous, with some insolubles. We have constructed cavities in the sedimentary beds and have had cases where the shale interbedding collapses and damages tubing and

one thing or another. We have had no case of collapse from the surface. There is a case in Kansas recently where a salt producing company had a very large cave-in. As I remember it made a hole at the surface of 500 feet in diameter and 150 feet deep. I don't know the exact figures. There was one case in Texas where a company was building a cavity in a salt dome that came up very close to the surface. They were building the cavity near the surface and they had a collapse and failure. Those two are the only ones I know of.

The time required for drilling and casing the hole and washing the cavity to 13 million barrels and to empty the brine is estimated to be 48 months for this minimum investment case. The cost is estimated to be about $2.5 million, exclusive of cost of site, site preparation, fresh water supply, utilities and brine disposal facilities, and hydrocarbon blanket.

I had mentioned the hydrocarbon blanket previously. It would be used for roof control. It would be 100 percent recovered. So although you might have an investment of several million dollars in hydrocarbon blanket during construction, it would all be recovered and used for other purposes. The cost of site and site preparation, fresh water supply and utilities and brine disposal would depend on the location, whether it was on the gulf coast or inland, and various other factors. It might run from 500,000 to a million dollars or more. A more detailed estimate of cost and time required for construction is given in table I.

Representative HOLIFIELD. This would make a total cost of about $3½ million?

Mr. MEADE. That is right.

Representative HOLIFIELD. For a 500-foot diameter hole.

Mr. MEADE. That is right.

Representative HOLIFIELD. That type of a hole could be used over and over for tests.

Mr. MEADE. That is a question of conjecture. I don't know whether it could or not.

TABLE I.—*Estimate of cost and time to develop 13,000,000-barrel cavity minimum cost—Case I*

	Cost-dollars	Time-months
Drill and case 1 hole and equipment for washing	225,000	2
Pumping facilities, 5,000 gallons per minute, 4,200 horsepower	750,000	
Wash bell hole for insolubles	1,225,000	4
Top cavity development		4
Wash to final size		22
Cavity shape surveys	25,000	1
Dewatering pump, 900 gallons per minute, 1,200 horsepower	150,000	1
Dewater cavity	205,000	14
Total	2,580,000	48
Engineering, 5 percent	129,000	

NOTE.—This estimate does not include the cost of site, site preparation, fresh water supply, utilities, brine disposal facilities, hydrocarbon blanket nor the time required for acquisition of these items.

Representative HOLIFIELD. In the Hardtack series we did use the same hole, as I understand, for more than one test.

Mr. MEADE. It would depend on what the explosion did to it. That I don't know. We will only find out by trying.

For the second case, that of minimum construction time, a similar technique would be used, the basic difference being that a number of holes would be drilled and cased concurrently. Let us assume for this case that four holes for washing are drilled on a 50-foot circle and that one hole of the same size is drilled in the center for shape control. This center hole would be used for injection of the hydrocarbon blanket and for running a continuous sonic survey on shape. The four wash holes would no longer need protective strings which would make it possible to increase the wash string to 13⅜ inches—34 centimeters. With this arrangement a circulation rate of 48,000 gallons per minute—3,030 liters per second—could be obtained which would require about 38,400 horsepower. The cavity would then be emptied with five of the pumps previously mentioned, one in each hole.

May I have the next slide, table II, please. The time required for drilling and casing the hole and washing the cavity to 13 million barrels or 73 million cubic feet—2.1 times 10^6 cubic meters—and to empty it of brine is estimated to be 16 months for this minimum time case. The cost is estimated to be $11,125,000 again exclusive of site, site preparation, fresh water supply, utilities and brine disposal facilities and hydrocarbon blanket. A more detailed estimate of cost and time required for construction is given in table II.

TABLE II.—*Estimate of cost and time to develop 13,000,000-barrel cavity, minimum time, case II*

	Cost—dollars	Time—months
Drill and case 5 holes and equip 4 for washing	1,125,000	2
Pumping facilities, 48,000 gallons per minute, 38,400 horsepower	6,850,000	
Wash bell hole for insolubles	2,190,000	4
Top cavity development		2
Wash to final size		4
Cavity shape surveys	30,000	
Dewatering pumps, 5, 900 gallons per minute, 1,200 horsepower	750,000	1
Dewater cavity	180,000	3
Total	11,125,000	16
Engineering, 5 percent	556,000	

NOTE.—This estimate does not include the cost of site, site preparation, fresh water supply, utilities, brine disposal facilities, hydrocarbon blanket, nor the time for acquisition of these items.

You will observe the large difference in cost between the two cases is brought about by having to drill the larger number of holes and install the much greater pumping facilities.

It must be realized that such a vacity could be constructed at a slightly lower cost than is given for case I by extending the time some more, and that the time for construction for case II could be reduced slightly by the expenditure of a considerably higher sum of money.

The 13 million barrels, or 73 million cubic feet (2.1×10^6 cubic meters) cavity which I have been discussing, is only slightly larger than some of those which we know have been formed by chemical companies. The construction of considerably larger cavities using the same techniques is believed entirely feasible.

For instance, a cavity of about 40 million barrels or 225 million cubic feet (6.4×10^6 cubic meters) would be about 750 feet (230 meters) in diameter. On the basis of criteria given, this cavity would

be of sufficient size to decouple a nuclear explosion of up to 90 kilotons or so. The next slide (table III), please. It could be constructed and dewatered with five wash holes as described in case II. Here we have the same investment as we had in case II except we are going to wash longer. It would be constructed in approximately 28 months at an estimated cost of $13,505,000. A more detailed estimate of cost and time required for construction is given in table III.

TABLE III.—*Estimate of cost and time to develop 40,000,000-barrel cavity*

	Cost—dollars	Time—months
Drill and case 5 holes and equip 4 for washing	1,125,000	2
Pumping facilities—48,000 g.p.m., 38,400 h.p.	6,850,000	
Wash bell hole for insolubles	4,140,000	4
Top cavity development		2
Wash to final size		10
Cavity shape surveys	50,000	
Dewater pumps 5-900 g.p.m., 1,200 h.p.	750,000	1
Dewater cavity	590,000	9
Total	13,505,000	28

Engineering, 5 percent, $675,000.
This estimate does not include the cost of site, site preparation, fresh water supply, utilities, brine disposal facilities, hydrocarbon blanket, nor the time required for acquisition of these items.

You will notice that this is only about $2½ million more than the 13 million barrel hole and is roughly three times as large.

While there are many conventionally mined operations in salt formations at various depths that are at atmospheric pressure, almost all of the solution-mined cavities have been maintained full of brine or hydrocarbon. This has been done to simplify operations. It is believed that cavities located in salt at a depth between 2,500 and 3,250 feet (760 and 990 meters) will have an initial closure in the range of 5 to 10 percent. The rate will be greatest at the beginning, however, and decrease with time. This problem has been evaluated and is discussed in detail in "An Evaluation of the Factors Influencing the Stability of a Large Underground Cavity," by Prof. Don U. Deere of the University of Illinois. This investigation was made for the AEC in October of 1959.

In conclusion, it can be said that cavities of the size that would decouple nuclear explosions up to 20 to 30 kilotons have been built and that it would be easy to construct cavities several times larger using the same techniques. These techniques are well known in the chemical and oil industries, and have been widely publicized. Large cavities could be easily constructed at a reasonable cost anywhere in the world that large deposits of salt are found.

Representative HOLIFIELD. Thank you very much, for your statement, Mr. Meade.

Representative VAN ZANDT. In concluding your statement, you say it would be easy to construct cavities several times larger than, as we understand it, those capable of concealing 20 to 30 kiloton nuclear explosions. Are we correct in assuming, based on your analysis at least, a 100-kiloton nuclear explosion could be concealed underground by the method you have just described?

Mr. MEADE. I believe, if I follow your question, that is correct. In other words, by the construction of a cavity of roughly 40 million

barrels you could conceal a nuclear explosion of up to 90 or 100 kilotons.

Representative HOSMER. Aside from this particular method you have described in creating caverns, there do exist in nature a great many subterranean caverns of large size?

Mr. MEADE. I am not too familiar with those, sir. I believe that an investigation was made last fall as to the natural cavities that were in existence. I am not sufficiently familiar with it to talk about it.

Representative HOSMER. Are there any salt dome cavities man-made that are abandoned?

Mr. MEADE. I believe there are. They are sitting there full of brine. I can't say whether they are abandoned or not. They are owned by various corporations.

Representative HOSMER. Out of use?

Mr. MEADE. They are out of use, I believe, because the companies have decided against making them any larger.

Representative HOSMER. Is this process for obtaining brine used in other countries than the United States?

Mr. MEADE. Yes.

Representative HOSMER. It is fairly worldwide?

Mr. MEADE. It is a well-known system of producing salt.

Representative HOSMER. Costwise is the Fraasch process for extracting sulphur a good deal more expensive?

Mr. MEADE. I can't discuss that. I don't know.

Representative HOSMER. Thank you, sir.

Representative VAN ZANDT. Mr. Meade, I think you are acquainted with a fact that a shot in outer space would cost about $20 million.

Mr. MEADE. No, I am not.

Representative VAN ZANDT. This information we have in our possession. Were we to use underground cavities we could not only instrument them better but could prepare them at a much more reasonable cost.

Mr. MEADE. It would appear so.

Representative VAN ZANDT. And we would eliminate, as Mr. Durham mentions here, the radiation hazard.

Mr. MEADE. That is right.

Representative VAN ZANDT. So underground tests in these cavities seem to be the best place to do the job.

Mr. MEADE. It looks very attractive. I know very little about it, however.

Representative HOLIFIELD. Your testimony has been very valuable to the committee, Mr. Meade. We appreciate your taking the time to respond to our invitation.

Mr. MEADE. Thank you.

Representative HOLIFIELD. Just a moment before you leave, please.

Mr. RAMEY. Was it clear from your testimony that you believe there are comparable deposits in the Soviet Union to those that you described in the United States?

Mr. MEADE. Yes, I do. I am not a geologist and prior to coming here I asked one of our geologists to make a brief literature survey on that very subject. In a matter of 3 hours time he came up with about 50 references to salt domes in the Soviet Union. I had my secretary type off two or three pages of them and did not think it worth her while to type any more. I might read you briefly a reference in 1939,

Bulletin of the American Association of Petroleum Geologists, volume 23, No. 4, page 492 to 516. They say: "Although oil has been produced since 1908 from salt domes in the Emba area which lies between the south end of the Ural Mountains and the north shore of the Caspian Sea, the geological situation in that region has escaped the attention of many American geologists. Salt dome geologists of the gulf coast region have frequently had their attention directed to the salt structures of Germany and of Rumania, whereas greater resemblance to the gulf coast is to be found in the Emba district where more than 100 salt domes are known in an area about the size of the gulf coast exclusive of south Texas. Future discoveries may bring the total to 300 or more salt domes, including the deep ones." Then they go on and tell about the oil production. I am sure a review of the literature will reveal a lot of salt domes in Russia.

Representative HOLIFIELD. Would you submit that list to the committee?

Mr. MEADE. Yes.

(The document referred to follows:)

FEASIBILITY OF CONSTRUCTING LARGE CAVITIES

By L. P. Meade

BIBLIOGRAPHY OF REFERENCES TO RUSSIAN SALT DOMES

[Bulletin of the American Association of Petroleum Geologists, vol. 23, No. 4, April 1939, pp. 492-516]

EMBA SALT—DOME REGION, U.S.S.R., AND SOME COMPARISONS WITH OTHER SALT—DOME REGIONS

C. W. Sanders, Houston, Tex.

Although oil has been produced since 1908 from salt domes in the Emba area, which lies between the south end of the Ural Mountains and the north shore of the Caspian Sea, the geological situation in that region has escaped the attention of many American geologists. Salt-dome geologists of the gulf coast region have frequently had their attention directed to the salt structures of Germany and of Rumania, whereas greater resemblance to the gulf coast is to be found in the Emba district, where more than 100 salt domes are known in an area about the size of the gulf coast exclusive of south Texas. Future discoveries may bring the total to 300 or more salt domes, including deep ones. Only six salt domes are being exploited but many others have shown evidence of accumulations of oil and gas. Production has been found in "Permo-Treassic," Jurassic, and cretaceous strata, but chiefly in Jurassic sands. The oil reserves appear to be commercially important but do not rank with those of the gulf coast.

The Emba mother salt series is of Permian age—probably Upper Permian. The overburden is much thinner than that of the gulf coast, consisting chiefly of Mesozaic strata, with local patches of Tertiary sediments, and a blanket of Quatermarz (Caspean) beds in the coastal area.

Many of the salt structures are larger than any gulf coast shallow domes. Future exploration will probably reveal an increasingly larger percentage of deep domes.

More is known about presalt strata in the Emba region than in the gulf coast because of the outcrops in the southern Urals. The Upper Carboniferous and the Permian sections have been rather closely correlated by several investigators with the Pennsylvanian and Permian sections of the Mid-Continent region, largely on the basis of the ammonites contained.

Development has been slow in the Emba district due largely to the remoteness of the area, the long severe winters, and the thinness of the sands as compared with the prolific Tertiary sands of the Caucasus belt.

The tectonic setting of the Emba domes is discussed and compared with that of the salt domes of the gulf coast, of the North German basin, of Persia (iron), and of the Carpathian foothills belt in Rumania.

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Aezenshtadt, G. E. A.

1. Vsesayus. Neft. Nauch.-Issled. Geol.-Razo Inst; Tr. V. 95 (Geol. Sbornik 2). pp. 21–30 illus. 1956.

In the Emba region, U.S.S.R., petroleum accumulations are associated with Meso-Cenozaic salt domes and with buried Permo-Treassic (or Upper Permian?) salt structures. A classification for the various types of accumulation is proposed, based on differences in mode of occurrence—strutegraphic and structural traps and other types of reservoirs.

2. Vsesayus. Neft. Nauch.-Issled. Geol.-Razo. Inst; Tr. V. 95 (Geol. Sbornik 2) pp. 214–221, 1956.

The salt domes of the southern Emba region, U.S.S.R., are classified into two major groups—primary salt domes (Permo-Treassic) and secondary domes (Meso-Cenozaic). Various types distinguished among the secondary domes are described.

Fridland, V. M. Page 171

Vaprosy Geog. Sbornik 33, pp. 261–275, illus., 1953.

A discussion of the occurrence of salt domes and saline soils in the Caspian basin U.S.S.R., with special reference to their interrelationships and their significance from the point of view of water supply.

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Aezenshtadt, G. E. A.

Geol. Sbornik (Vsesoyuz. Neft. Nauch.-Issled. Geol.-Razo Inst.) 3 (6) pp. 215–218 illus. 1955.

Presents new data on the tectonics of salt domes relating to the development of secondary domes on the flanks of the original structure. During uplift of the salt mass, the care migrates upward. With each new stage of uplift, recurrent movement takes place along the principal fault, and additional anticlinal structures are developed on the flanks of the domes.

Avrov. V. Ya. Page 29

Geol. Sbornik (Vsesoyuz. Neft. Nauch.-Issled. Geol.-Razo Inst. 3 (6) pp. 199–214, 1955.

An analysis of the processes responsible for the formation of salt domes in the Caspian region, U.S.S.R. It is concluded that their formation took place in pre-akchagylia time, during relatively intense Tertiary crustal movement, and that the nature of the overburden played an important role in their local development.

Luzhnaya, N. P. (and E. I. Lukyanna and F. A. Makarenko) Page 368

Vaprosy Geol. Azii (Akad. Nauk SSSR) v. 2, pp. 633–642, illus., 1955.

Discusses the origin and nature of the salts in the Uzbai dry water course, Turkmenistov, U.S.S.R.

Meshcheryakov, Yu., A. Page 400

(Gerasimov. I. P., ed) pp. 65–78, illus., discussion, pp. 79, 98, Akad. Nauk, S.S.S.R., Kom Izuchenizu Chetvert. Periola, Moscow, 1953.

Discusses evidence of recent tectonic activity in the north Caspian region, U.S.S.R., particularly that associated with intrusion of salt dome in the region. The surface expression of the various types of salt domes is analyzed, regional structures are described, and the principles used in evaluating salt-dome activity are outlined.

Shcherbina, V. N. Page 545

Akad, Nauk. Kirgiz, SSR, Inst. Geol., Frunze, 1956

A monographic study of the mineralogic composition, petragraphic characteristics, and mode of formation of continental Tertiary salt and gypsum deposits in the intermondane basins of the Tien Shan range in Kirgiz, Asiatic U.S.S.R.

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Aezenshtadt, G. E. A.

Akad. Nauk S.S.S.R. Daklady t. 68, no. 5, pp. 901–903, 1949.

An investigation of Jurassic sedimentation in the Emba salt dome region, western Kazakhstan, U.S.S.R. based on barehole data. No correlation was found between thickness or lithology of individual zones and the location of the salt domes, thus indicating that no sedimentation took place during periods of salt dome growth. Although the overall movement of the basin was one of subsidence, this was interrupted by periods of uplift accompanied by emplacement of the salt dome.

Vol. 19 Page 294
Lunin, P. I.
Akad. Nauk S.S.S.R. Inst. Geol. Nauk Tr. V 60, Inst. Geol. Nauk. Tr. V 60, Geol. Ser. No. 18, 34 pp (Russ. Engl. summ.) illus. 1941.
The development of Permian salt deposits in the Ural region, U.S.S.R., was controlled in part by the presence of buried structures in the subsurface of the depositional basin, crystal growth, which promoted the formation of originally slightly convex beds, and tangential pressures were also agents in the doming process. The buried structures in the salt dome area of the Ural-Emba district are oil-bearing; similar structures in the Ishimbaen district may prove to be petroliferous.

Vol. 17 Page 394
Sharapov, I. S.
Sovet. Geol. No. 5, pp. 44–52 sk. map., 1941.
Discusses the conditions of formation of the salt structures in the Dnieper-Donets basin, U.S.S.R., and concludes that they were developed during regional folding and are the product of intrusion of salt into higher zones from underlying Devonian anticlinal folds.

Vol. 16 Page 125
Guraru, F. G.
Acad. Sci., U.R.S.S., B., Ser. Geol. No. 4, pp. 75–88 (Russ., Eng. Summ) illus 1947.
Study of the distribution of Saline springs and the geological structure of the Olekominsk district. Yakutsk U.S.S.R., indicates that lower Cambrian rock salt masses occur as cores of the structural uplifts with which the springs are associated. The salt masses are believed to have played a considerable role in the formation of the structures.

Kolbin, M. F. Page 166
Moskov, Obshch. Ispyt, Priorody, B. t. 53 Otdel Geol. t. 23, no. 6, pp. 83–91, 1948.
Discusses new data on the geomarphology and structure of the right bank of the Volga river below Stalingrad, U.S.S.R., and their relations with gravity anomolies of the region, and concludes that the diversion of the river to the east is due to the emplacement of salt domes.

Vol. 13 Page 150
Kosyzin, Yu A.
(1) The Mechanism of the Salt-Dome Formation: Soc. Nat. Moscow B. t. 50, Sec. Geol. 5 20 (5–6) pp. 3–29 (Russ. Engl. summ.) 1945.
Low specific gravity, compaction, and plasticity of rock salt are its principal properties of tectonic significance the formation of salt dome is due to tangential pressure.
(2) Tectonics of salt uplifts in the Dnieper-Dometz depression: Soc. Nat. Moscow. B. t. 5, Soc. Geol. t 21 (3) pp. 39–64 (Russ. Engl. summ.) 1946.
The salt domes were formed on a basement of Hereynion (late Paleozoic) folds and maintained the strike of these folds during uplift in the upper cretaceous and lower Tertiary.
(3) Moscow Soc. Nat. B.V. 53, Geol. Soc. V. 23, No. 2 pp. 63–66. 1948.
Some fundamental features of salt dome distribution in the southern Emba region, U.S.S.R., The salt domes are located along the margin of the Baiehuma depression. The position and orientation of salt domes in the western Kazak uplift region are frequently determined by the mechanics of subsidence in the Caspian geosyncline.

Mohrinsky, V. V. Page 188
A Scheme of Inder-dome formation in Western Kazakhstan.
Intern. Geol. Cong. 17th, U.S.S.R., Rept. V. 2, pp. 641–644, 1 fig. 1939
Intrusion of salt domes in the Inder region, U.S.S.R., began in the upper Permian and continued into the post-Pliocene. Lower Permian salt beds were displaced and localized into dome-shaped uplifts.

Vol. 12 Page 68
Dzens—Litovsky, A. I.
Soc. Russe Miner. Mem. V. 69, No. 4, pp. 549–571. 1940.
Describes karst phenomena associated with the Iletsk salt dome, Crenburg district, U.S.S.R.

Vol. 10 Page 89
Mozharovsky, B.A.
Saratov, Univ., Geol. Inst. Tr. V. 1, No. 1, pp. 9–18 (Russ., Engl. Summ.) 1936
The Ural—Emba region, Russia, is a sloping uplift cut through by several salt domes and flanked on the west by the depression of the Volga-Ural steppes. The salt domes are the centers of development of highly mineralized waters, and the areas of tectonic depression are probably zones of accumulation of fresh or weakly mineralized artesin waters.
Shinayapin, P.A. Page 118
Saratov Univ., Genl. Inst. Tr. V. 1, No. 1, pp. 28–49 (Russ., Engl. Summ) 3 figs. 1936.
An account of the strategraphy (lower Permian to Pliocene) and tectonics of the Ozinki area in the Saratov district, Russia. Several salt domes occur in the area. The tectonic activity associated with their formation ceased during the Pliocene.
Vol. 9 Page 91
Fatiadi, E. E.
Eastern Oil, Moscow, V. 2, No. 9, pp. 21–26, 1940
New structural data based on study of gravity anomolies in the Volga-Emba oil and gas district, Russia. The district is located in the Caspian Sea depression in which many salt domes occur.
Mozharovsky, B. A. Page 204
Saratov, Chernyshevsky, State Univ. Inst. Geol., Tr. t. 2, vyp. 2–3, pp. 3–20 (Russ. Fr. summ. pp. 18–20), 1938
The northeastern part of the Saratov region, Russia, is composed of salt domes, probably of Jurassic age.
Shindyapin, P. A. Page 268
Saratov, Chernyshevsky State Univ.
Inst. Geol. Tr. t. 2, vyp. 2–3—pp. 21–33 (Russ., Fr. Summ) 1938
Describes the salt domes of the Ural River region, Russia.
Voitesti, I. P., 1926, Geology of the salt domes in the Carpathian region of Rumania: In geology of salt dome oil fields, R. C. Moore, Editor, Amer. Assoc. Petroleum Geologists, pp. 87–121.
Contains map showing rock salt masses.

Representative VAN ZANDT. Did I understand you to say a review of the literature would reveal a lot of cavities behind the Iron Curtain?

Mr. MEADE. It revealed a lot of references to them. Not cavities; salt domes. How many there are I don't know. I have not tried to go through the literature that we picked up and my knowledge of Russian geography is so limited that I don't know where they are.

Representative VAN ZANDT. From your statement you convinced me it is really possible to do some cheating when using these salt domes. If Russia has them, there may be some cheating going on.

Mr. MEADE. I think it is a well known fact that they have a lot of salt.

Representative HOSMER. It is said that in Siberia they have salt mines.

Mr. MEADE. The salt mines of Siberia are legendary.

Mr. RAMEY. Do you think it is possible to pick up evidence of pumping the brine out from such a hole that was made in the gulf?

Mr. MEADE. It would depend on what efforts were made to dispose of the brine. In producing a salt cavity we do not leave any traces of our brine. There are various governmental agencies that don't like brine dumped loosely in the rivers. We usually use disposal wells. We drill a well down to another formation which has salt water in it and inject the brine into that. A lot of the gulf coast cavities have the brine dumped into the Gulf of Mexico. You ask is it possible to detect it. It probably would be if you instrumented and searched for it. However, it apparently has not upset the equilibrium of the gulf enough so that anybody has noticed it so far. You must also

remember that a lot of brine is produced with oil. That brine, too, is dumped into the ocean.

Representative HOLIFIELD. At this point I have an article on nuclear test detection that appears in the March issue of Nuclear Information. This is a group of St. Louis scientists and citizens. It has been sent to Congressman Price, who represents that area, I guess, and also a copy is sent to me. It appears to be a technical article on the difficulties in digging a hole. The writer apparently is not aware of this type of mining. But at Congressman Price's request, unless there is objection, we will include it in the proper place in the record. (See attachment 4, p. 426.)

Representative HOSMER. Mr. Chairman, I would request that the matter be laid over for the present time.

Representative HOLIFIELD. It will be laid over. Would you have any comment on that?

Mr. MEADE. I reviewed it very briefly. It was handed to me yesterday. I am not acquainted with the writer and I do not know what experience he has had in constructing cavities in salt, as he suggests. It looks to me like it might be done that way but it is the hard way to do it. Since he suggests this method, it would seem to me that he is not at all familiar with the solution mining practice in common use in the chemical and oil industries. I don't think you would have the difficulty that he forecasts in disposing the brine in the gulf.

For instance, it seems to me that is the easiest place in the world to go ahead and build a cavity without detection. You would move in the bayou country of southern Louisiana on a salt dome that didn't have any oil and sulfur. You would set up a drilling rig that for all the world is an oil drilling rig. People know there are lots of oil wells in that country. After having completed it the rig is taken down, you have left there some pumps and other things that are often associated with oil wells. You might lay a pipe out into the gulf for several miles and lay the brine down on the bottom of the gulf, 50 or 60 feet below the surface. Due to the fact that it has a specific gravity of about 1.2 as contrasted to slightly over 1 for sea water, which is roughly one-tenth as saline, it would tend to stratify and lay on the bottom of the gulf in my estimation. We know of cases in coastal rivers which are tidal where the top layer may be eight or ten thousand parts per million of salt and the bottom will be up to 25,000 parts per million of salt, which is sea water.

Representative HOLIFIELD. Thank you very much, Dr. Meade.

(The full statement of Dr. Meade follows:)

A REPORT ON THE FEASIBILITY OF CONSTRUCTING LARGE UNDERGROUND CAVITIES

(By L. P. Meade)

The principal purpose of my discussion is to provide information on the feasibility of constructing large underground cavities. Such cavities might be constructed in various formations by well known conventional hard-rock mining methods. However, such a cavity would be very costly and would take quite a few years to build. It is also believed that many formidable difficulties would be experienced in such an undertaking. A large cavity might also be constructed in a salt formation by solution mining or leaching methods. While these techniques may not be as well known, they are simple and economical. A cavity constructed by these methods is estimated to cost only about one-tenth as much as one mined

in hard rock. I shall therefore restrict my remarks to the creation of large cavities in salt by solution mining techniques.

About 250 underground cavities have been constructed in salt in the United States for the storage of petroleum products—mostly liquified petroleum gas. A list of these cavities is given in the September 28, 1959, issue of the Oil and Gas Journal. Many more cavities have been formed by salt and chemical companies for the production of brine. For the most part, underground cavities have been formed by the solution mining method, because of much lower cost and simplicity of this method over conventional mining.

There are two types of salt formations in which cavities have been formed. These are sedimentary beds on the one hand and salt domes on the other hand. Salt domes are to be preferred because they are purer, more homogeneous and thicker than sedimentary beds. There are more than 200 salt domes in the gulf coast region of the United States from the Mexican border to western Alabama. These domes range in size from one half to 7 miles in diameter (0.8 to 11 kilometers) with an average of from 1 to 2 miles (1.6 to 3.2 kilometers). The average depth to the top of the salt is from 500 to 1,000 feet (150 to 300 meters). The average thickness of the salt is over 10,000 feet (3 kilometers) and in many cases is known to be as deep as 30,000 feet (9 kilometers). A map and listing of these salt domes is given in the August 11, 1952, issue of the Oil and Gas Journal. It is a well-known fact that similar salt domes exist in other parts of the world. A survey of the literature reveals references to a large number of areas in the Soviet Union where salt domes exist.

The cavities which have been constructed for petroleum storage range in size from 25,000 barrels or 140,000 cubic feet (4,000 cubic meters) to 1 million barrels or 5,600,000 cubic feet (160,000 cubic meters). Much larger cavities have been created by chemical companies for the production of salt. One such cavity in the Bryan Mound near Freeport, Tex., is located between the depths of 3,000 and 3,500 feet (915 and 1,065 meters) below the surface and has a maximum diameter of 500 feet (150 meters).

Phillips Petroleum Co. has constructed some 20 cavities in salt, 3 of which are in the Clemons Dome near Freeport, Tex. A fourth is now under construction in this dome. The largest is located between the depths of 1,910 and 2,260 feet (580 and 690) meters below the surface, is roughly cylindrical in shape, with a maximum diameter of 235 feet (73 meters). It has a volume of 1 million barrels or 5,600,000 cubic feet (160,000 cubic meters). The deepest cavity is located between the depths of 2,840 and 3,110 feet (865 and 950 meters) below the surface, is also roughly cylindrical in shape, with a maximum diameter of 150 feet (46 meters). It has a volume of 250,000 barrels or 1,400,000 cubic feet (40,000 cubic meters). These cavities were purposely made the shape and size indicated. It is easy to make them of other shapes and sizes by use of the same methods of construction. The size can be increased by leaching over a longer period of time or at a higher rate or both. The shape can be controlled by a technique involving the use of a hydrocarbon blanket for roof control and the point at which the leaching water is injected and the direction of circulation—whether from the top down or the bottom up. Repeated sonic surveys would be made as construction progressed to make sure that the cavity would be the actual shape desired.

In order to give you some idea as to the cost and time required for constructing such a cavity let us assume one of 13 million barrels or 73 million cubic feet (2.1×10^6 cubic meters) volume located 2,500 feet (760 meters) below the surface in a salt dome. This cavity would be about 516 feet (157 meters) in diameters if it were spherical. On the basis of criteria given, this size cavity would decouple the seismic signal of a 30-kiloton explosion. Cost of constructing and time for construction are closely interrelated. For this reason I will consider two cases, first that cost of construction is of considerable importance and is to be minimized, and second that time of construction is of paramount importance and is to be minimized.

For the first case, that of minimum cost, a single hole would be drilled and cased as shown by figure 1 (p. 140). A surface conductor 36 inches (91 centimeters) in diameter would first be set to a depth of about 100 feet (30 meters) and cemented to the surface. A hole would then be drilled into the caprock and a 30-inch (76 centimeters) surface casing would be cemented to the surface. Conventional oilfield cementing techniques would be employed. This casing is necessary to protect the water-bearing formations and to assure success in drilling through the caprock and on into the salt. The caprock is often porous making it impossible to maintain circulation of drilling mud which is necessary for the stabilization of the hole in unconsolidated formations above. The hole would then be

drilled on into the salt dome to the desired depth of 2,500 feet (760 meters) to the top of the cavity and a 22-inch (56 centimeter) casing cemented to the surface. The hole would then be drilled to its total depth of 3,400 feet (1,040 meters). A 16-inch (40 centimeter) diameter "protective" casing would be suspended to a depth of approximately 3,016 feet (920 meters) and a 10¾-inch (27 centimeter) wash string suspended to a depth of 3,400 feet (1,040 meters).

The washing operation would be started with the creation of a small space for the hydrocarbon blanket used for roof shape control. See figure 2 (p. 142). This would be done by pumping fresh water down the casing annulus and up the wash string. After this space is formed a light oil such as burning oil or kerosene would be injected into the casing annulus until returns were obtained in the protective string annulus. As long as hydrocarbon is maintained at this level leaching cannot take place above it. Circulation would then be established with fresh water going down the wash string and brine returned up through the protective string annulus. This would be continued long enough to create a bell hole of sufficient size to contain the 10 percent or so of insoluble anhydrite that would be released from the salt during the entire cavity washing operation.

As soon as the anhydrite space has been formed, the wash string and protective strings would be pulled to a point above the top of the cavity and the actual shape determined by a sonic survey. As shown in figure 3 (p. 143), the wash string would then be re-set approximately 500 feet (150 meters) below the top of the cavity and the protective string 100 feet (30 meters) below the top of the cavity. The hydrocarbon blanket would be spotted near the top of the cavity. Washing would be resumed with fresh water being injected down the annulus of the protective string and brine being returned up the wash string as shown in figure 3. Since the fresh water is lighter than brine, it would tend to spread out on the upper portion and dissolve salt from the top of the cavity. Additional hydrocarbon blanket would be injected from time to time to control the shape of the roof. After the top portion of the cavity has been formed the wash string would be pulled and a sonic survey run to check on the shape and size. The wash string would then be reinstalled and washing would be resumed with the fresh water being injected down the wash string and the brine removed up through the protective string annulus as shown in figure 4 (p. 145). With this system of washing, salt would be removed at a fairly uniform and rapid rate throughout the height of the cavity. Hydrocarbon would be added to the roof control blanket from time to time to maintain the desired shape. Sonic surveys would be run at intervals to check on the size and shape so that corrective steps could be taken when necessary.

With the strings of casing shown for this case a maximum circulation rate of approximately 5,000 gallons per minute (315 liters per second), could be achieved with a power requirement of about 4,200 horsepower. It is estimated that 33 months would be required to drill and case the well and wash the cavern to 13 million barrels or 73 million cubic feet (2.1×10^6 cubic meters).

Following completion of washing it would be necessary to empty the cavity of brine. This would be done by removing the hydrocarbon blanket by displacing it with brine and then pulling the 16-inch (41 centimeters) and 10¾-inch (27 centimeters) casing and installing a submerged motor electric pump through the 22-inch (56 centimeters) casing. This pump would have a 1,200 horsepower motor and be suspended on a 10¾-inch (27 centimeters) tubing through which it would deliver 900 gallons per minute (57 liters per second) to the surface. At this rate it would take 14 months to empty the cavity.

The time required for drilling and casing the hole and washing the cavity to 13 million barrels or 73 million cubic feet (2.1×10^6 cubic meters) and to empty it of brine is estimated to be 48 months for this minimum investment case. The cost is estimated to be $2,580,000 exclusive of cost of site, site preparation, fresh water supply, utilities and brine disposal facilities. These items will depend a lot upon location, but might run in the order of $500,000 to $1 million. A more detailed estimate of cost and time required for construction is given in table I (p. 146).

For the second case, that of minimum construction time, a similar technique would be used, the basic difference being that a number of holes would be drilled and cased concurrently. Let us assume for this case that four holes for washing are drilled on a 50 foot (15 meter) circle and that one hole of the same size is drilled in the center for shape control. This center hole would be used for injection of the hydrocarbon blanket and for running a continuous sonic survey on shape.

The four wash holes would no longer need protective strings which would make it possible to increase the wash string to 13⅜ inches (34 centimeters). With this arrangement a circulation rate of 48,000 gallons per minute (3,030 liters per second) could be obtained which would require about 38,400 horsepower. The cavity would then be emptied with five of the pumps previously mentioned—one in each hole. The time required for drilling and casing the hole and washing the cavity to 13 million barrels or 73 million cubic feet (2.1×10^6 cubic meters) and to empty it of brine is estimated to be 16 months for this minimum time case. The cost is estimated to be $11,125,000 again exclusive of site, site preparation, fresh water supply, utilities and brine disposal facilities. A more detailed estimate of cost and time required for construction is given in table II (p. 147).

It must be realized that such a cavity could be constructed at a slightly lower cost than is given for case I by extending the time some more, and that the time for construction for case II could be reduced slightly by the expenditure of a considerably higher sum of money.

The 13 million barrels or 73 million cubic feet (2.1×10^6 cubic meters) cavity which I have been discussing is only slightly larger than some of those which we know have been formed by chemical companies. The construction of considerably larger cavities using the same techniques is believed entirely feasible. For instance, a cavity of about 40 million barrels or 225 million cubic feet (6.4×10^6 cubic meters) would be about 750 feet (230 meters) in diameter. On the basis of criteria given, this cavity would be of sufficient size to decouple a nuclear explosion of up to 90 kilotons or so. It could be constructed and dewatered with five wash holes as described in case II in approximately 28 months at an estimated cost of $13,505,000. A more detailed estimate of cost and time required for construction is given in table III (p. 148).

While there are many conventionally mined openings in salt formations at various depths that are at atmospheric pressure, almost all of the solution mined cavities have been maintained full of brine or hydrocarbon. This has been done to simplify operation. It is believed that cavities located in salt at a depth between 2,500 and 3,250 feet (760 and 990 meters) will have an initial closure in the range of 5 to 10 percent. The rate will be greatest at the beginning, however, and decrease with time. This problem has been evaluated and is discussed in detail in "An Evaluation of the Factors Influencing the Stability of a Large Underground Cavity" by Prof. Don. U. Deere of the University of Illinois. This investigation was made for the AEC in October of 1959.

In conclusion it can be said that cavities of the size that would decouple nuclear explosions up to 20 to 30 kilotons have been built and that it would be easy to construct cavities several times larger using the same techniques. These techniques are well known in the chemical and oil industries, and have been widely publicized. Large cavities could easily be constructed at a reasonable cost anywhere in the world that large deposits of salt are found.

Representative HOLIFIELD. Our next witness is Dr. Edward Teller. He will testify on additional techniques of decoupling. Following Dr. Teller, Dr. Hans Bethé, a consultant to the President's Scientific Committee, will discuss the decoupling theory and other aspects of concealment. Dr. Teller, we are happy to have you before us again. You may proceed to make such statement as you wish.

STATEMENT OF DR. EDWARD TELLER,[1] LAWRENCE RADIATION LABORATORY, LIVERMORE, CALIF.

Dr. TELLER. Mr. Chairman, gentlemen, it is a very great pleasure to be here with you.

You have heard that due to the excellent and ingenious work of Dr. Albert Latter we now have a reliable method which has been since tested by experiment and which can give a decoupling by a factor of 300 as compared to the standard Rainier coupling which had formed the basis of earlier discussions. This factor of 300 is greatly in excess of any improvements which had been suggested and which it will take a long time to carry into practice.

Dr. Latter has also emphasized that this decoupling by a factor of 300 is not necessarily the strongest decoupling which could be accomplished. It is merely the simplest kind of decoupling, that kind of decoupling for which a purely theoretical estimate could be carried out and about which everybody is in full agreement.

There are other possible procedures of decoupling, all of them based in one way or another on the presence of some sort of cavity. Therefore, the testimony which you have just heard from Mr. Meade continues to be relevant for the things that I am saying.

What causes the earth movement which seismographs detect at a great distance is the pressure pulse which arises in the volume of the cavity. The cavity may have been there originally or it may have been produced by the nuclear explosion itself.

There is an extremely simple relation, simple and not quite correct, but for a point of reference I will quote it. You take the pressure in this cavity and multiply it with the volume of the cavity and you get roughly the energy released by the nuclear explosive. Now it is not the energy really which causes the earth's motion, but the pressure which is related to the earth's motion. There are other things that energy can do apart from causing pressure in a gas. Energy can also evaporate substances that one has put into that cavity. Energy can also heat substances without evaporating them. There exists a sug-

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Education: Karlsruhe Technical Institute, Germany, 1926–28; University of Munich, 1928–29; Leipzig, Ph.D., 1930.

Positions: Research associate, Leipzig, 1929–31; research associate, Gottingen, 1931–33; Rockefeller fellow, Copenhagen, 1934 (with Niels Bohr); lecturer, University of London, 1934–35. Professor of physics, George Washington University, Washington, D.C., 1935–41. Professor of physics, Columbia University, 1941–42. Physicist, Manhattan engineer district, 1942–46; 1942–43, University of Chicago; 1943–46, Los Alamos Scientific Laboratory. Professor of physics, University of Chicago, 1946–52. Los Alamos (on leave, University of Chicago), 1949–52. Consultant, assistant director, Livermore Branch, University of California, Lawrence Radiation Laboratory, 1952–53. Professor of physics, University of California, 1953. Associate director, Lawrence Radiation Laboratory, University of California, 1954. Director, Lawrence Radiation Laboratory, University of California, Livermore, 1958.

Research: Dr. Teller's research interests are broad, and he has made important contributions in several fields—in chemical physics, molecular physics, nuclear physics, and quantum theory. He was an early researcher in studies of thermonuclear reactions—the processes by which stars like the sun generate energy. In recent years he has attracted attention for his role in the practical application of thermonuclear principles in the development of thermonuclear weapons. He has made contributions to the spectroscopy of polyatomic molecules and in recent years has maintained an interest in the theory of atomic nucleus.

Honors: Doctor of science, Yale University, 1954, University of Alaska, 1959, Fordham University, 1960.

Awards: Joseph Priestley Memorial Award, Dickinson College, Carlisle, Pa., 1957, Albert Einstein Award, 1958, General Donovan Memorial Award, 1959.

Memberships: National Academy of Sciences; American Nuclear Society Fellow, American Physical Society; American Academy of Arts and Sciences; Scientific Advisory Board, Air Force; General Advisory Committee, Atomic Energy Commission; American Ordnance Association.

gestion, a very old one which several of us made from time to time and on which some crude calculations are in progress. This suggestion is that we put into the cavity some substance which can be evaporated and in so evaporating will use up some of the energy. Thus a smaller fraction of the energy will be left over to make pressure and thereby to move the walls of the cavity.

The latest proposal is to evaporate a substance which is rather hard to evaporate; namely graphite. It is entirely likely, although not proven, that by this means the decoupling factor can be increased from the factor 300 to a factor 1,000 or 3,000, or conceivably more.

Furthermore, this absorption of energy into evaporating substances also makes it possible to use smaller cavities. It will reduce the critical size of the cavity at which the cavity is big enough. Therefore, we can get more decoupling without excessive expense. Instead of evaporating the substances we may merely heat them and possibly melt them. If one has the cavity filled with some sort of iron filings, one could lead the energy into the iron. The energy which is absorbed in heating on melting this iron will not appear as pressure. One has to see how quickly the heating can be accomplished and how long the pressure will persist before the iron takes up the excess heat. What really causes the earth's motion is not just the pressure but the pressure times the time during which this pressure has been acting. This is again only an approximation, but by letting the pressure act only for a short time and then dumping it into some heat sink one again can reduce the size of the signal.

All of this is hard to calculate and I cannot tell you here that thereby it will be possible to increase the decoupling factor threefold or tenfold or more. I suspect so, and I think the only reliable procedure is to find out by experiment. Unfortunately, in this case nuclear experiments are greatly preferable to high-explosive experiments. The best heat absorbing substances seem to be those which need the highest temperatures and which will not be evaporated by high explosives. Also when you try to use iron filings as an energy sink, the problem is to get the energy from the explosive to the metal, to the heat sink. This can be accomplished in the case of nuclear explosives more expeditiously than it can be accomplished in the case of high explosives. Therefore, to see whether these future improvements in decoupling will or will not work, nuclear experimentation will be necessary.

This does not exhaust by any means the list of possible methods of concealment. It does not even exhaust the more limited list of methods of which I know or of which I hear. There is the possibility of multiple explosions which are not aimed at decreasing the strength of the earth's shock, but which are aimed at changing the characteristic shape of the earth's movement, so that it should be more easily confused with genuine earthquakes. Or one can so arrange the explosions that it will be more difficult to locate the nuclear explosion.

By placing several nuclear explosives around a central point and then detonating them simultaneously, one obtains a shorter travel time from each to the periphery The result will be that the explosion seems to come from the center but the signal has arrived in a shorter time than expected. Earthquake waves are faster when they are moving in the lower layers of the earth and therefore this arrangement might give the impression that the explosions happened deeper down. We always have agreed that an explosion that appears to happen at a greater depth than 10 or 20 miles is not likely to be manmade.

You have heard that nuclear explosions, and generally explosions, are apt to give an earth movement outward as the first phase. This phase is then followed by an inward movement, the inward movement being quite a bit stronger than the first outward movement has been. One can precede a decoupled nuclear explosion with a much smaller explosion, decoupled or undecoupled, in such a way that this much smaller explosion will make a positive outward-going signal which is too small to detect. But its bigger inward-going signal can coincide with the outward-going signal of the nuclear explosive and compensate it or overcompensate it. In this way it can wipe out the characteristic signature of a nuclear explosion. It is therefore by no means clear whether or not nuclear explosions can be identified as such even if lots more work is done on the subject and even if we have a very closely spaced net.

I certainly believe that more research on detection is necessary. We should push ahead this wonderful science of seismology. Had we known more about it we would have made fewer mistakes in the past months. Seismology is a great science in which further improvements are possible but because a lot has already been done in seismology further improvements may not come very easily. The study of underground nuclear explosions is a much more recent technical subject. This is the reason and not our ingenuity which has made it possible to find out more about the behavior of nuclear explosions and in particular about the possibilities of decoupling than it has been possible to find out about better seismological methods.

My hunch is that further developments may continue to go in the direction that we shall learn even more about concealment and that it will be quite difficult for methods of detection to catch up with methods of concealment.

I should, however, like to make one point quite explicitly. If you want to be optimistic, and if you want to believe that methods of detection can be worked out so that tests can be really controlled and checked and policed, I dare not say that this is impossible. Very few things in science are impossible. But I daresay that we shall have to improve presently available methods very manifold, and this will take a lot of money and it will take a lot of time. I do not believe that there is any great likelihood that in 2 years from now, or even in 4 or 5 years from now, there will be a really foolproof method of checking underground explosions down to, let us say, 1 kiloton. If we should be successful in this very different enterprise, it will take a time of many years and an amount of money of very many millions of dollars.

Now I would like to conclude with a very brief word about space testing. I am convinced from studies which have been pursued that space testing is entirely feasible. It is more expensive than underground testing, but not necessarily more expensive than the kind of test operations which we performed in the Pacific. Because of the great number of precautions which we had to take, these tests ran into many millions of dollars. Space testing could be extremely important when we try to test big explosions. The fact that you perform tests in a vacuum gives rise to some difficulties but can also give rise to some advantages. There is no doubt that in space testing the lifting power of rockets is a very essential point, and the Russians clearly are ahead of us in the lifting power of rockets.

I would like, however, to end here with a word of caution. I wholeheartedly agree with the possibilities of space testing that have been laid before this committee. I agree with the possibilities of observations of space testing, and with the possibilities of evasion of these observations which Dr. Panofsky and Dr. Richard Latter explained to you in great detail.

With all of that I agree. They have done work which is outstanding and which is done as well as it could be done. But such work cannot be done satisfactorily. Today we know as much about space testing as we had known about underground testing before 1957 when the first underground shot was fired. Since that time we have made many pitfalls, we have corrected some of our errors, and we have a little perspective how much more can be learned by experimentation

I think if we talk seriously and conscientiously about policed disarmament then a great deal of experimentation, underground and in space remains to be performed before a decent technical judgment can be delivered.

Representative HOLIFIELD. Thank you, Dr. Teller.

Are there any questions?

Representative VAN ZANDT. Doctor, as I recall it you were in favor of underground testing years and years ago.

Dr. TELLER. This is correct.

Representative VAN ZANDT. Had we followed your advice we would not have this problem of contamination which concerns the world.

Dr. TELLER. We have advocated underground testing for a number of reasons. One of them was getting away from contamination. One of them was easier test procedures. One of them was and is that underground testing opens a door into peaceful applications. And one very strong reason was the increase in our technical knowledge that comes from performing a new experiment in a new medium.

Representative VAN ZANDT. Doctor, I took the time to read your testimony before the Humphrey subcommittee back in 1958. There are just a few questions I would like to ask you about your statement.

At that time you mentioned that large uncertainties in test detection imbalanced detection knowledge in favor of detection and little information on cheating. Is that your position today?

Dr. TELLER. I am afraid I am a conservative. I did not find it possible and I am stupid enough not to have found it necessary to change my point of view.

Representative VAN ZANDT. You have made yourself clear since 1950 on several times that we can detect large tests but not small ones.

Dr. TELLER. We can detect very large tests underground if they are not hidden, and very large tests indeed even if they are hidden. In space with our present setup we can detect nothing. The Russians may be testing in space at any size right now.

Representative VAN ZANDT. At that time you said there are many other ways to hide tests. I imagine you had reference to underground.

Dr. TELLER. I meant underground hiding and I meant hiding in space. At that time we had been discussing the big hole theory but we did not have the correct answers. The correct answers were given by the ingenious work of Dr. Albert Latter, who should get very full credit for that. We have, however, discussed these questions at that

time and we have also discussed space testing. All this was secret at that time and was therefore not included in my testimony.

Representative VAN ZANDT. Doctor, do you feel that since this moratorium went into effect of stopping all tests that we as a Nation may have lost some of our nuclear advantage?

Dr. TELLER. If you want to assume that the Russians have used the opportunity of clandestine testing, we may have in that period lost much or all of our advantage if we had it in the beginning. I think it is likely that as of 1958 we were ahead of the Russians. But I also believe that our knowledge on the point who is ahead of whom is fragmentary, and I would not be sure about that point. But about this I am sure; if the Russians have used the intervening interval for continued testing during the time that our hands were and still are tied, they could have made very great progress. In every 2-year period in the past the Russians had made great progress and I don't see why in the last 2-year period they should not have done likewise.

Representative VAN ZANDT. Would this apply specially in the field of small nuclear weapons?

Dr. TELLER. This could apply more easily to the field of small nuclear weapons than to anything else because explosions of small nuclear weapons can be hidden without any trouble whatsoever.

Representative HOLIFIELD. If the gentleman will yield, as far as the record shows, however, we must make it clear that we have no information that the Russians have utilized this period for testing, is that not true?

Dr. TELLER. As far as I know we have no information whatsoever that they have or have not utilized this period. We know definitely that, had they tested, there would be no technical means by which we could have picked it up. We could have picked it up only by intelligence. We know also that our intelligence has failed to warn us in 1949 of the impending first Russian nuclear shot. Whether our intelligence has improved in the meantime sufficiently so that absence of information from intelligence can give us reassurance that now the Russians are indeed not testing, about this I have no special knowledge.

Representative VAN ZANDT. In other words, you are not making the charge that Russia has cheated?

Dr. TELLER. I certainly do not. I only said if they had elected to cheat they could have done it. I think that every one of you gentlemen is in a much better position to evaluate the probabilities whether or not Russia is likely to cheat when undetected.

Representative VAN ZANDT. If they had cheated they could have destroyed the advantage that we as a nation enjoyed, especially in the development of the small nuclear weapon.

Dr. TELLER. This is entirely possible.

Representative VAN ZANDT. That is all, Mr. Chairman.

Representative HOLIFIELD. Mr. Hosmer.

Representative HOSMER. Dr. Teller, disregarding the minimizing techniques of decoupling and the various other techniques you mentioned, with equipment for detection that could now be put together, what is the minimum size underground explosion that could be detected with certainty?

I am not talking about the 180 station system that has been talked about at Geneva. I am talking about devices that are better than that.

Dr. TELLER. I really cannot tell. This is an extremely difficult problem and I will try my best to answer in order to make it quite clear that I am not evading the question. We have been worrying about that question a great deal. I do not want to be taken seriously on this point. The most obvious improvement would be to put in a very great number of satellite stations. I think that even though this is expensive, if it is politically feasible I would be strongly for it. Obviously if one makes the net fine enough one could detect very small explosions indeed. For instance, the network that has been recommended of 250 miles distance, could detect according to the evaluation of Dr. Richard Latter, as you have heard, 18 tons. That is only a crude estimate. Eighteen tons of undecoupled, of fully coupled explosion, which would mean more than 5 kilotons if it is decoupled. If further methods of decoupling are developed, then even this narrow net might fail to pick up anything under 20 kilotons.

However, when we try to talk about this very narrowly spaced net, we are talking about many thousands of events, among which we have to pick out the suspicious ones. We know little about the signatures, the characteristic natures of earthquakes and how they differ from the characteristic signatures of explosions. When you go into the characteristic signatures of these very small earthquakes there might be even more confusion. Furthermore, there is a possibility of distorting the signature as I have mentioned to you.

The method that can be pursued, but which takes a lot of work, a lot of money, a lot of inspection—thousands of inspections, would be a very fine net.

Beyond that you have all the wonderful proposals of the Berkner panel about which you have had competent testimony and I cannot improve on it. These are the developments of seismology which ought to be pursued. One should go deep underground and see what noises you find there. You should find the detailed earth's motions and you should find the characteristics of earthquakes. All these are hopeful things. But none of them are likely to pay off in a time shorter than a few years. If we wanted to put in such a fine net, which I say we can do, even in that case it will take a couple of years to put in that net because it is such a big enterprise. I don't know, Mr. Hosmer, whether I was responsive at all.

Representative HOSMER. I take it when we add minimizing techniques we confuse the issue. Whatever knowledge we have now just cannot give us a meaningful answer.

Dr. TELLER. I think the answer must be at present that no matter how we proceed with the help of present knowledge we cannot control nuclear explosions of the tactical weapon size and perhaps not even some experiments which can serve as useful models for bigger explosions. But certainly we cannot police the tactical nuclear explosions. How this will change in coming years—whether, as Senator Jackson has asked, even bigger explosions can be hidden in the future or whether the methods of detection will win out I cannot say. We cannot find out except by trying and we are at the very beginning of trying.

Representative HOSMER. I take it for most of the nuclear powers the high-yield technology is better known than the low-yield-technology and as a consequence if they were cheating it is apt to be in the low yield field anyway where the difficulties of detection are greater.

Dr. TELLER. I would think this is likely. I should perhaps say that in the last years the interests of my associates and my own interests have increasingly and in an easily documented fashion turned more and more to the lower yield explosions. At the same time, I would be quite reluctant to say that we know enough about the high yield nuclear explosions. I know of important improvements that could be carried out in that field, too.

Representative HOSMER. I have one other question I would like to ask Dr. Teller. That has to do with the 180 station system as you understand it to be. We have had a lot of testimony about instrumentation and the obtaining of signals but we have not had anything about how anybody is going to communicate out of those 180 stations to some central headquarters anything that might be a suspicious manifestation. Can you throw any light on the system?

Dr. TELLER. Sir, this seems to me a question a little bit out of my field. It is not really a technical question now but a question of human communications.

Representative HOSMER. We can ask somebody else. (See footnote 6, p. 12, Summary—Analysis of these hearings.)

Dr. TELLER. I would say that on the difficulties or the possibilities of honest cooperation with the Russians I feel particularly unqualified to comment. I have not even been present at the Geneva discussions so I do not have the firsthand experience which quite a few of my colleagues have. So I am very reluctantly in the position where I feel that it is wiser if I leave this question unanswered.

Representative HOSMER. Let me ask you this question, from the standpoint of the purpose of the control systems, is the importance of communication, which is certain, equal to the importance of detection which is certain?

Dr. TELLER. I think that the communications problem is extremely important. I think that the problem of accessibility is extremely important. Even after you have located a suspicious event and you send out your people, and even after they are allowed to go there, the problem of finding the radioactivity is an exceedingly difficult problem. The best way of finding it is probably by intelligence methods. Wherever you have intelligence methods, however, you also have the possibilities of Russian counterintelligence. How this game of intelligence and counterintelligence will influence those people who go out on inspection duty, what will be the possibility of communicating to us whether that has been a suspicious event, all these questions are outside of my technical field.

Representative HOSMER. Thank you.

Representative HOLIFIELD. Senator Gore.

Senator GORE. The last response, Doctor, is what prompted my question, but I will ask it anyway.

Are you a seismologist?

Dr. TELLER. No.

Representative HOLIFIELD. You said a few moments ago that the committee had received competent testimony. Were you referring to the seismologists who testified in their particular field?

Dr. TELLER. I referred to the seismologists who testified in that field. I also referred to Mr. Meade and I referred very particularly to Dr. Latter. Seismology is an important and interesting field about which some of us have learned a few things. But also our

own field, that of nuclear explosions and decoupling of such nuclear explosions with earth movement, this also is a field which I would call seismology to a certain extent but it is something into which seismologists had no occasion to look in the past. Here is a field in which very simple physicists like Dr. Latter and Dr. Bethé and Dr. Martinelli, many others, and myself, have started out perhaps on an equal footing with the seismologists. What will be the effect of a nuclear explosion on the immediate neighborhood of the earth's motion? I think you have listened to expert testimony by, for instance, Dr. Latter.

Representative HOLIFIELD. Will you please identify which Dr. Latter you are speaking about?

Dr. TELLER. I am talking now about Dr. Albert Latter.

Representative HOLIFIELD. I asked that question for the record because two have testified.

Dr. TELLER. That is right. Even though some of us are not seismologists our testimony with respect to decoupling is quite relevant because we are pushing into a new field where we can make contributions which I hope in the course of time will be incorporated into seismology.

Senator GORE. Thank you, Mr. Chairman.

Representative HOLIFIELD. Mr. Durham.

Representative DURHAM. Doctor, we always enjoy your lectures and there are many we have heard in the past few years. They are always very, very interesting and have sound reasoning. I remember the days when you were advocating underground tests a long time ago. I have letters in my files from you on this very, very subject.

Representative HOLIFIELD. If the gentleman will yield, I think the record should show that the gentleman from North Carolina, Mr. Durham, in early 1958 made public statements advocating the underground testing.*

Representative DURHAM. Thank you very much, Mr. Chairman.

I might say that we are facing pretty soon probably some kind of developments in this field in coming to a treaty. Now is it your opinion at the present time science has advanced or provided the adequate means to identify nuclear tests under present conditions:

Dr. TELLER. I think that science has not advanced to that point. With respect to this treaty, from the point of view of a technical person I would like to make this much of a recommendation. If the United States signs a treaty in which there are adequate safeguards against cheating, this I think is a clearcut situation with which I certainly would feel that as a scientist I ought to agree. If we want to sign a treaty in which it is clearly stated that there is no attempt at control, or in which it is clearly stated that there is an attempt at control but this attempt is and will remain for the next decade in all likelihood open to extremely serious doubt, so that actually now and for the next decade we will not have policing, if for reasons of politics into which I should not and cannot enter, this is deemed to be right, as a scientist and as a technical expert again I cannot comment and I cannot object to it. But if we enter into a treaty where the pretense is made that we can be checking, where the fact that there are really great loopholes and possibilities of evasion, where these facts are not put clearly before the American people, then I feel that I must raise my voice as a scientist in protest.

*Remarks by Representative Carl T. Durham, (Then) Chairman of the Joint Committee on Atomic Energy, before meeting of the National Association of Manufacturers, New York City, May 21, 1958.

Representative DURHAM. In other words, you feel that the only way to do this is either by manned stations by some human being instead of by machines at the present time under present conditions.

Dr. TELLER. I did not mean to say that at all. I think if you have a sufficiently close network of very many unmanned stations, properly constructed—and I don't know what properly constructed means but I have a little confidence that this might be worked out—under those conditions there might be some hope. However, I see enormous difficulties along that line, too, because then the number of events which would have to be inspected becomes very great. The number of events goes into many thousands. I do not see how a sizable chance of real detection and identification would arise.

Prior to the close investigation of this thing, I feel bound to state that we have no system.

Representative HOLIFIELD. Do you advocate a high priority program of research and development in seismological and other electronic device field:

Dr. TELLER. I feel that there should be a higher priority program in seismology, in methods of detection, and also in methods of evasion. Because if you restrict yourself to one, then you are instructing me to play chess, but to move the white pieces only.

Representative HOLIFIELD. Do you believe that program should be under the independent control of the United States:

Dr. TELLER. I have the feeling that there would be no technical or other reason that I can see why such a program should not be undertaken jointly with any number of nations who would want to participate in it. Many nations have indicated their passionate interest in test control. It would seem to me that we well might utilize their outstanding scientific talents, whether they be Russians or Germans or British or anyone else, to help and cooperate in finding out the methods by which detection and evasion is possible.

Representative HOLIFIELD. There have been three methods suggested: Completely independent program in which we would not be dependent upon any other nation for funds and for instruction. A coordinated program in which we could pursue our own independent course and make known to other nations the value of it, and which they could also make known to us what they know. Then a joint program which would depend upon unanimous agreement and be subject to a veto. That is why I asked you the question in the way I did.

Dr. TELLER. If you put the alternatives in this way, and you ask my opinion which is clearly not just a technical opinion, I would like to see as much international cooperation as is possible without obstruction from anybody.

Representative HOLIFIELD. Then you would advocate a coordinated program?

Dr. TELLER. I would think that a coordinated program where we do what we want to do, invite everybody to participate in it, tell everybody what we find, but do not wait for anybody's permission to go ahead with the program, this would seem to me to make sense.

Representative HOLIFIELD. Do you think the present program of research and development in this field is adequate in view of the Berkner report and in view of the urgency with which you think it should occur?

Dr. TELLER. I believe the Berkner report's recommendation of $30 million has been too modest. It has also been much more than we have actually spent.

Representative HOLIFIELD. I realize that. It is much higher than what we are are spending at the present time.

Dr. TELLER. I think we have here an issue on which our national security may well depend. I think that the support of this program is woefully inadequate.

Representative HOLIFIELD. Then you would advise a stronger research and development program?

Dr. TELLER. Most definitely.

Representative HOLIFIELD. In this field?

Dr. TELLER. Most definitely.

Representative HOLIFIELD. Mr. Westland.

Representative WESTLAND. Dr. Teller, just one question: Suppose one nation were to conduct these undetectable tests and the other nation did not, what could the results be?

Dr. TELLER. Let me put it this way, Mr. Westland: The development of nuclear weapons has been represented, and I should say misrepresented, in the public mind as occurring in big jumps, in big breaks. In 1945 we knew how to make an atomic explosion. In the early 1950's we learned how to handle the hydrogen bomb. Everything else does not matter.

I think this kind of description is completely wrong. What has in fact happened is that each year we have improved our methods by a considerable factor which in the course of years had a most impressive cumulative effect. We know that the Russians have proceeded with a speed certainly not less than ours. If they continue testing and we do not continue testing and if the lessons of the recent past mean anything, then they will overtake us within a very short time. Is this responsive to your question, sir?

Representative WESTLAND. Yes, that is partially in response, Doctor, that they would overtake us in that area. It seems to me if one nation were to conduct tests and the other did not, the one that did not might very soon be confronted with a situation where there would be no defense.

Dr. TELLER. I think this is the way I see it; yes, sir.

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Doctor, in 1958 when you were before the Humphrey subcommittee, you said this:

> The only way to insure adequate inspection is unfettered entry into Soviet Russia with complete freedom to interrogate people, unhindered access wherever needed.

Are you still of that opinion today?

Dr. TELLER. I am very much of that opinion.

If I may, I would like to add that this thought is not at all one to which I lay particular claim. The greatest physicist of our times, Niels Bohr, has advocated for many years, that the only really fruitful way toward any disarmament is the way toward complete openness. Once we have accomplished that, then the way toward disarmament and toward peace lies open before us. Without it, however, we are just inventing one method after another how to fool ourselves.

Representative VAN ZANDT. Mr. Chairman, one more question: You also said back in 1958, Doctor,

Test detection systems have so far been directed only against tests which no one tried to hide.

Does that hold true today?

Dr. TELLER. I feel that I cannot give a very simple answer to that. I would say test detection systems today are effective only against such tests which no one tries to hide. We are trying to direct them now against tests which people try to hide, too. We are trying, but so far I don't see that success is very near.

Representative VAN ZANDT. Are you sure that we have all available information on types of tests that have been tried?

Dr. TELLER. Sir, I am not sure. One could easily say: The world is divided into two parts—that is what is above and what is below the surface of the earth. This looks like a complete enumeration—surface tests, underground tests, space tests. But there may be particular tricks. There may be surprises in any of these fields of which we have not thought. We tried to think and one very easily can fall into the misconception that one did think of all possibilities. I feel that probably we did, but I have been fooled so often that I dare not say yes.

Representative HOLIFIELD. Thank you very much, Dr. Teller.

Representative BATES. Mr. Chairman, I would like to pursue that question a little bit further. In response to Mr. Westland's question concerning the possibilities for the future. From the experience that I have had, the risks that we have been taking is in the perfection of weapons that we presently have. That is to make them smaller, but still have the same amount of power.

Do you see anything else on the horizon that we should investigate and develop besides that type of thing?

Dr. TELLER. I would say yes. If you ask me that I should tell you in detail what improvements are possible, this, of course we would have to do in executive session. I would say, however, that in addition to producing the same bang in a smaller size there are clearly other possibilities. Clean weapons have been discussed and more flexible weapons have been discussed. There are possibilities of making weapons in such a manner, of making them more adaptable to the particular task which they are to perform, so that they do not overkill, so they don't destroy where you don't intend destruction. If you look into the richness of the progress which has been made in the past, I certainly do not think that even these things that I have enumerated are sufficient. We have to develop weapons which are cheaper and which can be more available. All of these things can become important, not when we are talking about all-out warfare and not when we are talking about a first strike, but when we are talking about a second strike and about the safety of our retaliatory forces and also when we are talking about limited nuclear warfare. This is a particularly knotty problem about which we have just started to think in real detail.

Representative HOLIFIELD. Thank you, sir.

Senator GORE. Mr. Chairman.

Representative HOLIFIELD. Mr. Gore.

Senator GORE. Though I am not a member of the subcommittee, you have given me an opportunity to participate, which I appreciate. Since I cannot be here tomorrow and further since I made a statement yesterday in the hearing with respect to the communique of the Geneva

Conference on March 19, I ask permission to make a 4- or 5-minute statement here before having to depart.

Representative HOLIFIELD. Proceed.

Senator GORE. President Eisenhower made a proposal which was submitted to the Conference on February 11. I have before me a statement of the State Department describing that proposal. I will leave it here for all to see. I will read these two sentences.

> We believe the proposal placed before the Conference today if entered into in good faith by the parties will lead toward eventual prohibition of nuclear weapons tests under the practical and adequate safeguards that we deem as indispensable prerequisites. If accepted the proposal will be entered forthwith under assured controls.

I will not take the time to read all of it, but I will leave it here. The conditions are adequate inspection, assured controls. The proposal suggested 20 to 30 inspections in Russia per year. The Russians made a counterproposal on the 19th. Now our officials in Geneva and in Washington should have been forewarned that this was an effort to achieve a propaganda victory, because the Conference which was scheduled for Friday the 18th was canceled by the Russians and then they called for a meeting on Saturday. This is not the first time they have made a play for the Sunday press in the United States and around the world. I would like to read three paragraphs from the New York Times on Saturday, March 19. The story is dated Friday the 18th:

> The Soviet Union is expected to announce tomorrow its readiness to accept a limited treaty on the banning of nuclear weapons tests provided that it is coupled with a moratorium on tests not formally outlawed by the treaty.

I am skipping a few paragraphs and then I read as follows:

> Zarobin, the Soviet delegate, asked for the unusual Saturday meeting after he agreed earlier to the cancellation of today's session and the fixing of the next meeting for Monday. In addition to breaking into the weekend, Mr. Tzarepkin announced that he planned to hold a news conference after the session tomorrow.

So this was clear notice to everyone that here was a major Russian propaganda attempt.

Now I would like to read the official communique which I described yesterday and earlier as misleading, and which, incidentally, Mr. Philip Farley in a conversation with me acknowledged to be misleading, and which another official of high status acknowledged to be misleading. I would like to read the second and third paragraphs from this communique issued jointly by the three delegations:

> The Soviet delegation submitted a proposal to conclude a treaty on the cessation of all nuclear weapons tests including all underground tests producing seismic events of magnitude 4.75 or greater embodied in the U.S. proposal on February 11.

I digress. They did not propose to accept the conditions embodied in President Eisenhower's proposal. Yet that is the impression left here by this communique, I submit.

Now I return to read the third paragraph:

> The Soviet Union accepted as well the U.S. proposal on carrying out a program of joint research and experiments by the U.S.S.R., United States, and United Kingdom, provided all parties to the treaty simultaneously undertaken not to conduct during this period any nuclear weapons tests producing seismic events of a magnitude of 4.75 or under.

I submit that the question of misleading words are these:

"To conclude a treaty", second, "embodied", and third, "accepted as well."

Now what is the result of this communique and Mr. Tsarapkin's conference? I hold here Sunday's Washington Post. Here is a big headline, "Reds accept Ike's test plan provided small blasts cease."

"The Soviet Union offered today to sign a treaty proposed by President Eisenhower to ban all big three nuclear tests except low-powered explosions underground."

I digress. They did not propose to accept the treaty proposed by President Eisenhower. The "if" referred to here was not the condition of inspection or control.

The second paragraph:

> The big condition was that the United States and Britain must join the Soviet Union in a promise to refrain for an indefinite period from conducting the small underground blasts. This would be an honor system arrangement unpoliced.

What were some of the conditions which Mr. Tsarapkin only not refused to accept, but specifically rejected? One was inspection. Second was the composition and procedure of the control commission. The next was the fixing of a number of inspections. He specifically rejected any relationship between physical or scientific events or occurrences of suspicious events and the number of inspections. He said that must be a political question and he has not yet agreed to accept even one.

He also insisted that whatever quota may be agreed to, if ever agreed to, would apply to the small, below the threshold, suspicious events, as well as those above. Now he did not even agree to accept the President's proposal on a research program because the President specifically proposed that it would be necessary to use nuclear detonations. Mr. Tsarapkin specifically specified chemical and chemical only.

I am convinced after hearing Ambassador Wadsworth that he acted with the best intentions. I know he has labored patriotically and loyally. It was an unfortunate mistake in the choice of words which gave to the Russians a propaganda advantage around the world. I asked Ambassador Wadsworth to be careful hereafter, and he said he would.

I thank you, Mr. Chairman.

Representative HOLIFIELD. Our next witness will be Dr. Hans Bethé. He is one of the top scientists in America. He was a consultant to the President's Science Advisory Committee and is at the present time. He formerly was a member of the committee.

Dr. Bethé, you will have all the time you wish and you may testify on any point that you wish at this time.

STATEMENT OF DR. HANS A. BETHÉ,[1] CORNELL UNIVERSITY

Dr. BETHÉ. Thank you very much, Mr. Chairman. I want to testify on some purely scientific subjects; namely, I agree to a very large extent with some of the remarks Dr. Teller has made as to the most promising avenue for improving the detection system.

He said, and I agree, that the one avenue for improvement of the detection system which can be foreseen fairly definitely at this time is to put in a large number of stations. I wanted to give you a few numbers concerning the improvement that could be expected from such a closer net of stations.

You have heard large numbers of decoupling—300. I agree with this number of 300, and I made my own calculation to verify this number, and I presented the theory to the Russians at the Geneva Conference.

I want to point out that similar numbers could be expected and, in fact, are known to exist in the strength of the seismic signal when you go to closer spacing. I will write down some numbers on the blackboard which are obtained from the measurements made by Dr. Romney and his associates in the Hardtack II series.

I will just write down two numbers; namely, at 1,000 kilometers distance, the signal observed from the Blanca explosion which was 20 kilotons was 5 millimicrons. At 400 kilometers the corresponding number was 400 millimicrons. At 200 kilometers the number is over 3,000 millimicrons.

You will see that going from 1,000 kilometers to 200 kilometers you get an improvement by a factor of about 600. This is more than the decoupling factor. Therefore, I want to submit that at least technically it is possible to overcome the large factors for decoupling which we have learned about.

Now let me write down the numbers which would be expected from a decoupled explosion; and just to stay in the same ball park I would like to talk about a 20-kiloton explosion fully decoupled; that is, decoupled by the full factor of 300.

This requires just the size hole which Dr. Meade has talked about in his previous testimony. Such an explosion would give——

Representative HOLIFIELD. Dr. Bethé, would you make it clear which size he talked about? He mentioned several different size holes.

Dr. BETHÉ. This is the hole of 500 feet. You do not need quite that size. This size is actually good for 30 kilotons. But 20 kilotons is a number which has become standard. It is the nominal bomb. The Nagasaki bomb was of this size. It is the size tested in Nevada in 1958.

So I want to stick to 20 kilotons. I realize that the size of the hole is a little bit too big for 20 kilotons.

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Now, for a fully decoupled bomb of 20 kilotons, the numbers which you would expect are about 25 at 200 kilometers and about 3 at 400 kilometers.

Representative HOLIFIELD. You are speaking of three stations?

Dr. BETHÉ. No. This is three millimicrons of signal. This does not mean anything to you. This could not very well mean anything to you. I should tell you that the significant thing is the noise which you expect at the stations.

The noise at an average station is about 5 millimicrons when you use one seismograph; and when you use 100 seismographs, the noise becomes about 0.8 millimicrons. Therefore, you can see that at 200 kilometers you get a very big signal (25 millimicrons) compared to the noise which is easy to see.

At 400 you get a signal four times the size of the noise which you just about see, but not more. These results are obtained if we use the same instruments which we have been using in the past—namely, the Benioff seismographs, which have a maximum response at about three cycles per second.

However, when you go after the detecting game, you clearly want to do it as best you can. You want to make use of the characteristic of the signal which you expect from a decoupled explosion. Such a decoupled explosion has the characteristic that its signal is mainly in high frequencies. The earth is jiggled for a relatively short time. Therefore, the signal which is propagated in the earth will be one that starts and very soon dies out and, therefore, has a very short period.

Therefore, you will have a much better chance of observing a signal from a decoupled explosion if you use higher frequency. Now, why haven't we used higher frequencies before? We have not used higher frequencies largely because the high frequency short waves are very greatly weakened as they travel through the earth.

It is known in seismology how they are weakened—at least approximately—and if you talk about a high frequency signal with a period of one-tenth of a second, then it just will not travel to the distances like 3,000 kilometers at which most of the Geneva stations are located.

Therefore, the Geneva system has to rely on seismographs of relatively long periods and of low frequency because the high frequency signal will not go very far. But if we could have a system in which the stations are closer to the event, then we are free to choose higher frequencies which we know are generated in much larger intensity by the decoupled explosion.

If you go to a 10-cycle-per-second detection, then the energy which is fed into the signal increases about thirtyfold over that which we are recording with our present system, and at the same time—and this is very fortunate—on the average, the noise energy—that is, the energy of the wiggles which exist anywhere in the earth—decreases as we go to higher frequency, at least on the average, and under average conditions.

Representative HOLIFIELD. Do we have seismographs now that will record the high frequency waves?

Dr. BETHÉ. There are some such seismographs, but this science is in a very early stage. So this is something which requires a good deal of research. I think it is not very difficult research, but a good deal of research.

Especially this would be required in order to make sure of the noise. What I said was that on the average the noise is very good. These

waves of 10 cycles per second are very quiet. But if the wind blows hard then the noise becomes rather strong. It is likely that if you dig into the earth to some moderate depth—100 feet or a couple hundred feet—you will improve the situation greatly.

But this is something that has to be investigated and where we cannot predict the outcome at the present time. If I assume that I can use the 10-cycle-per-second and that the noise is of the average noise—I am not requiring a particularly quiet station, but just an average station—then I can make a system which has the following characteristics:

It will give you a signal 10 times the noise. That is an easily recognizable signal, which is obvious to anybody at eight stations from a fully decoupled 20-kiloton shot, and this system will require that I have a station at every 200 kilometers.

I think everybody realizes that 200 kilometers is a small spacing.

Representative HOLIFIELD. 120 miles.

Dr. BETHÉ. 125.

Representative HOLIFIELD. And that compares with what distance that is being considered?

Dr. BETHÉ. In seismic regions the present spacing is 1,000 kilometers; five times this.

Representative HOLIFIELD. About 600 miles.

Dr. BETHÉ. 600 miles.

Representative HOLIFIELD. So in terms of numbers of signals, or rather, stations——

Dr. BETHÉ. It would multiply it by about 25.

Representative HOLIFIELD. It would go up about 25 times as much.

Dr. BETHÉ. About 25 times. I figure this would need about 600 stations in Russia.

Representative HOLIFIELD. That is in the Soviet Union?

Dr. BETHÉ. In the Soviet Union.

Representative HOLIFIELD. Against the 20 that has been talked about.

Dr. BETHÉ. I think 21 is the number at the moment; 600 is a large number of stations. Of course, by saying this number, it is clear that something has to be done to make this number acceptable. This is a political question and no longer a technical question. The stations have to be made acceptable both to the Russians and to ourselves.

You have heard from other witnesses and have read in the Berkner report that it is proposed to make these stations unmanned and then, of course, you have to see to it that they are tamperproof and that you cannot easily reverse the polarity in a station.

Representative HOLIFIELD. Are you confident that this can be done? And if so, in what time period?

Dr. BETHÉ. I am confident that this can be done. I can only make a vague estimate of the time period. I would say 1 to 2 years. I would think that by the end of the period which we would need to install the big stations, we would probably know about the small stations. Maybe a little earlier.

Representative HOLIFIELD. It would probably take longer than that to do the research and development and also the manufacture of the number that is needed and to install them.

Dr. BETHÉ. The 1 to 2 years I meant just for the research and development. I would propose that after one has developed such a station, we put in an experimental station somewhere in this country and work with it and see how it works and that we then try to fool that station; that is, there be special teams delegated to——

Representative HOLIFIELD. To attempt concealment?

Dr. BETHÉ. Not concealment, but to attempt destruction of the capability of the station; that is, turning around the polarity or making noise and so on.

One would have to learn from such experiments just what one can do and what one cannot do against such spoofing.

Representative HOLIFIELD. Then I would ask you the same question I asked Dr. Teller. Do you advocate a high priority program of research and development in the seismological field and electronic field in order to improve our capability for detection?

Dr. BETHÉ. I certainly do.

Representative HOLIFIELD. Do you think that should be a program under which we would have full control; making known the information to other nations?

Dr. BETHÉ. I think we should have one program which is entirely under our control and where we should make the information available to all other nations. We should have a concurrent program in which other nations can participate. I know that it is always very difficult to make many people pull in the same direction, and for this reason I would say that we should have some program which is entirely under our control.

Representative HOLIFIELD. In other words, it would not be subject to veto or obstruction by any person or any nation.

Dr. BETHÉ. That is right.

Representative HOLIFIELD. You think it should go on a high priority basis, too?

Dr. BETHÉ. Yes, I do.

Representative HOLIFIELD. Do you think our present program of research and development is adequate in this field?

Dr. BETHÉ. No; it is by no means. It has taken a very long time since the Berkner report which was issued about a year ago before anything was done for implementation of the research program which was recommended.

Representative HOLIFIELD. You might be interested to know that this committee has been trying to push an adequate research and development program. Do you know the number of projects that are now being performed in the research and development field?

Dr. BETHÉ. I am afraid I do not know.

Representative HOLIFIELD. Do you know how much money is now being spent?

Dr. BETHÉ. Not even that do I know. It is several million.

Representative HOLIFIELD. We will get some information on that point later on. The Berkner report, as I remember, advocated about $30 million a year; did it not?

Dr. BETHÉ. Yes.

Representative HOLIFIELD. I understand that the present program is about $8½ million a year.

Dr. BETHÉ. Yes, I am sure you are right.

Representative HOLIFIELD. Those figures are subject to correction later.

Dr. BETHÉ. Yes. Of course, we must remember that this got underway only in the last couple of months. There was not anything last fall.

Representative HOLIFIELD. We are going to find out what has been done in this field. We will have later witnesses that will testify as to what contracts have been let with what universities, how many people were employed in this research and development program, and we will try to find out the status of it and also the schedule of planning for the next few years.

Dr. BETHÉ. Good.

Representative HOLIFIELD. As you testified, it has been over a year since the Berkner report came out.

Dr. BETHÉ. Yes. You may inquire as to the cost of such a net of small stations and here I want to repeat what Dr. Romney already said before, namely, that the best estimate which has been made so far is a hundred thousand dollars per station. This includes communications and includes some safeguards against tampering of various kinds. If it is not proof against tampering then, of course it, would be much cheaper. At this rate the whole system in the Soviet Union of these small stations would cost $60 million, which is not a small sum, but also is not a large sum. It is a small sum compared to the requirements for a detection system in space.

If we want a detection system in space, this will be far more expensive.

Representative HOLIFIELD. I might say that the $60 million that you estimate is about 60 percent of the cost of one Polaris-type submarine.

Dr. BETHÉ. Yes.

Representative HOLIFIELD. The submarine runs a little over a hundred million. That is one submarine.

Dr. BETHÉ. I am all for Polaris submarines, however. Supposing we had such a system, what would we do with the information? You will recall that I required that the signal be 10 times the noise. I required such a large signal in order to be able to see first motion in addition to just seeing the signal. We know from the experience of the Hardtack series that the first motion increases relatively much more if you go to small distances than does the signal afterwards. So that we should really be quite well off in regard to first motion.

Dr. Richard Latter mentioned in his testimony this morning that there would be some 18,000 events above an equivalent yield of 18 tons—tons, not kilotons. What I am aiming at in my exercise here is a somewhat more modest goal. I am aiming at 20 kilotons divided by 300, which is about 70 tons. That is, I want to detect a normal explosion, a coupled explosion of 70 tons, or an uncoupled or decoupled explosion of 20 kilotons.

Representative HOLIFIELD. That would be in tuff, and would require roughly three times that amount.

Dr. BETHÉ. No. This would be 20 kilotons decoupled in salt, which is equivalent to 70 tons coupled in tuff.

Representative HOLIFIELD. Decoupled in salt?

Dr. BETHÉ. Yes, decoupled in salt. At this level I estimate that the number of earthquakes in Soviet Russia would be about 5,000. I would hope that with a rather good system of detecting first motion we could identify 90 percent of these, which would leave about 500.

Five hundred is still a very large number. However, you would obviously not try to inspect 500 seismic events. After all, we are interested in the decoupled 20 kiloton explosions, and these decoupled 20 kiloton explosions can be easily performed only in salt. Only in salt beds can we easily use Mr. Meade's method of solution mining. Therefore, one would inspect first of all only those events which happened to originate from a place where there are salt beds. Now, although salt domes are widespread and there are hundreds of them in the Soviet Union, as we hear, they are not all over the place. I don't know what fraction. We once made an estimate which was that it was quite a lot less than 1 percent of the Soviet Union which would be eligible for such excavations. Hopefully saying 1 percent, this would leave you with only five events. You might in addition have some evidence other than the siesmogram which tells you that somebody has done a major operation (after all, this excavating of the salt is a major operation) in such and such an area. If they want to excavate these cavities near the Caspian Sea, let us say, this further restricts the area in which we need to look. If they excavate somewhere else, then it won't be so easily possible to put the brine into the sea. So I believe that it is quite possible to select by other evidence than just the seismic signal those signals which originate from places where there might possibly be a fully decoupled shot. When you come down to 10 or 20 events a year of this type from which signals have been sent and which are in regions where you could have a salt cavity, then you get a perfectly manageable problem of inspection.

I want to make one remark about the partial decoupling which Dr. Albert Latter has talked of—the results of the Cowboy explosions.

Representative HOLIFIELD. By the way, were you using the data of the Cowboy explosions of 300 into 20,000 where you arrived at the 70 tons?

Dr. BETHÉ. Yes.

Representative HOLIFIELD. But that was for chemical. Do you apply it with equal consideration to nuclear?

Dr. BETHÉ. It is generally believed that the decoupling factor between tuff coupled and salt decoupled is 300. This should be independent of chemical or nuclear. We must admit that this ratio has not really been measured. What is lacking still is a chemical explosion in tuff to verify that the salt decouples relative to tuff even when you have no hole. That is, even when you have a fully tamped explosion.

On the partial decoupling, I think we must believe the results from the Cowboy explosions. So it appears that you can get partial decoupling by a factor of 30 with a much smaller volume than you need for full decoupling. I somewhat disagree in the interpretation from Dr. Albert Latter. I think it is a matter of definition to some extent. But the volume which is required for this partial decoupling is one-tenth the volume which we previously specified, in my opinion, rather than one-thirtieth. This is still a very big volume.

Now, finally, a word about the capability to detect tests which might be further decoupled.

Representative HOLIFIELD. I might mention that some of the members of the committee had to go for a rollcall.

Dr. BETHÉ. Some of the schemes for further decoupling have been previously discussed. I think they are in a completely different category from the primary scheme of decoupling by a big hole. When

the big hole theory was first advanced by Dr. Albert Latter, I immediately recognized that this was a sound theory. I did my own calculations and confirmed his results. I do not feel at all the same way about the further decoupling possibilities. They depend on rather complicated schemes. One of the schemes that Dr. Teller mentioned is to distribute graphite dust in the big hole. This method was presented at the Geneva Conference. It didn't draw any particular fire from the Russians. But unofficially our friend, Dr. Penney, the head of the British delegation, pointed out to us that he had done experiments on very similar devices for quite different purposes and that they did not work at all. The difficulty is that this carbon dust must not be present before the nuclear explosion, because if it is present then it is just swept by the nuclear exposion to the wall. It is thereby rendered ineffective. In addition it increases the initial pressure spike which you always get when the shock from the air hits the wall. So you must not have the carbon dust before the nuclear explosion. So you are asked to so something rather complicated. You are asked to suspend little balls of carbon dust, let us say, at quite frequent intervals in a big cavity, explode them all simultaneously, and explode them at a time after the nuclear explosion passes them, but still early enough to absorb the heat from the nuclear explosion. What Dr. Penney pointed out is that he had tried to explode little balls of carbon dust, and instead of being nicely distributed in space he found that it all came together and was therefore useless. That doesn't mean that all these schemes will not work. I only want to leave with you the impression that these further decoupling schemes are by no means as simple as appears on the surface and by no means as sure. It is my opinion that the next round ought to go to the detection rather than to the concealment.

Representative HOLIFIELD. But if you admit, Doctor, as you have that the 300 to 1 decoupling process, which is the simple decoupling process, is possible, that in itself offers quite an obstruction to detection, does it not?

Dr. BETHÉ. It certainly does. I have not disputed this at all. I have talked about a system of stations which, if acceptable on both sides, could deal with these decoupled explosions. I know full well that these decoupled explosions would not be detectable by the normal Geneva net. On the other hand, in spite of the very beautiful testimony of Mr. Meade, it is a major undertaking to make such a hole.

Representative HOLIFIELD. But if those holes already exist, Doctor, they could be utilized, could they not?

Dr. BETHÉ. Holes already exist for 2 or 3 kilotons; not for 20.

Representative HOLIFIELD. 500 feet?

Dr. BETHÉ. There was one 500-foot hole. I don't know at what depth that was. Maybe we should inquire into that further. But this apparently is a rarity.

Representative HOLIFIELD. What size weapon would that take?

Dr. BETHÉ. If it is at the depth of about 3,000 feet, as we have always postulated, then it would take 30 kilotons.

Representative HOLIFIELD. If you had a 200-foot hole—I understand he said there were several of those—what size weapon would that take?

Dr. BETHÉ. This is between 1 and 2 kilotons. I should say 2 if it is at 3,000 feet depth. This is according to the full decoupling criterion, of course. It would take 20 kilotons with partial decoupling.

Representative HOLIFIELD. In other words, it could take 20 kilotons and still register below 4.75?

Dr. BETHÉ. It would certainly register below 4.75. It would register about as a 1 kiloton explosion in that case, which the Geneva net can detect but not identify.

Representative HOLIFIELD. Of course, a 20-kiloton test would certainly be of value from a military standpoint in testing the new configuration or new type of tactical weapon, would it not?

Dr. BETHÉ. For tactical weapons it would surely be quite effective. In fact, we talk of tactical weapons, let us say, from 20 kilotons on down.

Representative HOLIFIELD. Down to 1 or 2?

Dr. BETHÉ. Or fractions of a kiloton. Whether it would be effective for developing higher yield weapons is another matter. I believe that progress in modern high yield weapons would be very difficult on the basis of such tests. Modern high yield weapons.

Representative HOLIFIELD. Do you have any further statement?

Dr. BETHÉ. I want to talk about tactical weapons. Very much has been said about this. I think people largely disagree as to their usefulness. I want to make just one remark in this connection. If both we and the Russians had used tactical weapons in the Korean war, we could never have landed, because the thing which is most easily destroyed is port facilities. I do not believe that tactical weapons if used by both sides, as they would necessarily be, are more useful to us than to our opponents.

Representative HOSMER. In amphibious warfare, dock facilities were largely immaterial.

Dr. BETHÉ. I thought we had used the ports to quite some extent. It would also be quite easy to attack a landing party in the normal methods of amphibious landings. But I really don't want to make any judgment about this.

Representative HOSMER. That is outside the field of our inquiry, anyway, is it not?

Dr. BETHÉ. Yes. There are many arguments on both sides. I just wanted to point out that there are quite a lot of arguments on the side that it would help the Russians rather than us with our long communications.

Representative HOSMER. I am rather intrigued by your short wave detection system. Incidentally, if you have answered any of these questions while I was away at the rollcall, just tell me and I will not pursue them.

Is this system being used at any place as of the present time?

Dr. BETHÉ. I have answered this question. This is only in a very experimental stage at the present time and it has not been used by the detection system which we have used for the Hardtack observations.

Representative HOSMER. It is only in the beginning of research and development and evaluation?

Dr. BETHÉ. Yes.

Representative HOSMER. I take it that you feel that inherent in it are much more possibilities of meeting adequate detection requirements than in a long wave system.

Dr. BETHÉ. I think both the very short and the very long waves are very useful. The very long waves have been investigated par-

ticularly by Dr. Oliver, and are quite promising for discrimination between earthquakes and explosions. Quite a number of suggestions have been made and some have been incorporated in our Geneva documents. The very short waves have come into prominence only recently. One of the calculations that our group at the Geneva Conference, Technical Working Group II did, seemed to show was that we would be much better off in detecting first motion if we used higher frequency.

Representative HOSMER. Were you suggesting this as an additional set of stations or an alternate set of stations?

Dr. BETHÉ. The high frequencies are useful only when you are close to the explosion because they are absorbed very strongly by the earth.

Representative HOSMER. Are you recommending that we install both the long and the short wave?

Dr. BETHÉ. The long waves are useful at large distances. I would imagine that the short waves would be installed in small stations which we might have at every 200 kilometers, whereas the long wave devices would be installed at the big stations—at the 21 stations which we have in the present Geneva system in Russia.

Representative HOSMER. Then the answer to the question is "Yes," you are suggesting both a long wave and short wave integrated system.

Dr. BETHÉ. That is correct. The long wave system has already been accepted by the negotiating parties in Geneva. It is part of the recommendation of Working Group II that long wave seismographs be installed at all control stations.

Representative HOSMER. But the long wave system standing by itself is inadequate to detect cheating. Therefore, as a consequence you suggest the addition of the short wave system, too?

Dr. BETHÉ. Yes, that is right. The present system has long waves and medium waves. I suggest the addition of short waves.

Representative HOLIFIELD. If the gentleman will yield, would the 600 figure that you gave us for stations be in addition to the short wave or would that include both short wave and long wave?

Dr. BETHÉ. That would just be the short waves.

Representative HOLIFIELD. Then we would have 21 of the long waves.

Dr. BETHÉ. Or preferably 30 as Dr. Richard Latter recommended.

Representative HOLIFIELD. Are you contemplating these short wave stations having a hundred element array?

Dr. BETHÉ. No. I assume that they would have a single seismograph.

Representative HOSMER. It would more or less depend on how much background noise you had in the area?

Dr. BETHÉ. Yes. I figured the background noise according to the available data and then assumed just one seismograph. If you had several seismographs you could do better.

Representative HOSMER. You specified though that you want a signal that is in strength 10 times the background noise.

Dr. BETHÉ. That is right.

Representative HOSMER. If you are going to have a bigger background noise you will have to put in a bigger array.

Dr. BETHÉ. Yes. I took that background noise as observed with present equipment at this short wave.

Representative HOSMER. You have the background noise in your calculations?

Dr. BETHÉ. The background noise has been measured and I have taken the average background noise and I pointed out that when there is wind you have to take special precautions which we can only find by further research.

Representative HOSMER. Doctor, since you said there was not any of these short wave systems in being, how have you been able to determine what the short wave background noise is for this system?

Dr. BETHÉ. No system is in being but single instruments exist.

Representative HOSMER. Single instruments exist?

Dr. BETHÉ. Yes.

Representative HOSMER. Has a check been done on the United States and we know what the background noises are all over the United States?

Dr. BETHÉ. I believe this was not all over the United States but only at special points.

Representative HOSMER. Then you would have to go in and check the background noises.

Dr. BETHÉ. You certainly would.

Representative HOSMER. At each of these 600 stations we are talking about, they would have to be tailored to the background noise characteristic of the location.

Dr. BETHÉ. One would try to find a location of relatively low background noise but this might take some time.

Representative HOSMER. This requirement that you have established of signal 10 times background noise, does that apply to the long wave detection stations, too?

Dr. BETHÉ. No. This applied only to the short wave detection.

Representative HOSMER. What is the minimum requirement in the long wave station?

Dr. BETHÉ. That depends on what you want to see. I should first of all make the difference between real long wave, which means waves of 10 second period and more and medium wave, which means one-third of a second. The medium wave is the mainstay of our present detection system. When Dr. Richard Latter made the investigations of the requirements, he required 10 to 20 times background noise for these. If you want to detect first motion you need something like 10 times background noise at small distance and 20 times at larger distance. If you talk about the really long waves, the figure has not been so carefully specified so far.

Representative HOSMER. Did you discuss how many unrecognizable manifestations that would be turned up by the shortwave system?

Dr. BETHÉ. Yes.

Representative HOSMER. Is that worldwide or in the Soviet Union?

Dr. BETHÉ. Five thousand in the Soviet Union detected. I estimated about 500 of these unidentified.

Representative HOSMER. You mentioned some type of a communication system from these unmonitored stations. What did you have in mind?

Dr. BETHÉ. I am afraid I am out of my depth there. A study is being made at the moment by very competent people of the Sandia

Corp., and Bell Telephone Laboratories, and these are very great experts in communications, as you know. I believe they suggest cables but I am not sure. I think we have to wait until that study is completed.

Representative HOSMER. Will you answer this question, then? If the system is to accomplish its purpose, the integrity of the communication system must be at least equal to that of the detection system. Is that true or false?

Dr. BETHÉ. That is absolutely true.

Representative HOSMER. Does this represent a problem of roughly equal magnitude to that of developing the detection machinery?

Dr. BETHÉ. I do not know, but I don't think so. Communications, after all, is a well-developed art.

Representative HOSMER. You did, however, mention the necessity of building in some tamperproofing.

Dr. BETHÉ. Yes.

Representative HOSMER. You mentioned also the probable use of land lines. That would pose a considerable problem. I think you are capable of answering that, are you not?

Dr. BETHÉ. I am afraid I am not able to answer that. One would have to ask communication experts.

Representative HOSMER. In case we do not have any testimony along that line, Mr. Chairman, I think the record of the hearings should recognize the fact that we have another problem of equal magnitude to the one we spent 3 days discussing in this committee.

Thank you, Doctor.

Representative HOLIFIELD. Do you have any further testimony, Dr. Bethé?

Dr. BETHÉ. I have nothing further.

Representative HOLIFIELD. I might announce to the audience that Dr. Bethé will be on the panel tomorrow and we will welcome you back again at that time, Dr. Bethé. Thank you very much for your testimony.

Dr. BETHÉ. Thank you.

Representative HOLIFIELD. We are going to have to make a little change in our program in the morning. Capabilities for detecting tests in the atmosphere and high altitudes and space have been pretty well covered. In view of the fact that item No. 5, "Procedures, capabilities, techniques, and limitations of onsite inspections," should normally follow the type of testimony we have had, we will omit item No. 4 for the present and we will have item No. 5 in the morning. Then the panel. Then we will get to No. 4 later. So if Dr. Panofsky and Dr. Latter and Dr. Byron B. Leonard and Dr. Richard Taschek are in the room they might confer with the staff on this matter. If not, the staff will contact them.

The committee will resume tomorrow morning at 10 a.m. in room 312 of the Old House Office Building. We are unable to get this room. During tomorrow morning's session the committee wants to have the panel meet to discuss the capabilities of monitoring underground nuclear tests during a moratorium. We may have to call them back again after the high-altitude testimony.

At the present time the committee is adjourned.

(The following letter and supplementary material were received from Dr. Bethé:)

LABORATORY OF NUCLEAR STUDIES,
CORNELL UNIVERSITY,
Ithaca, N.Y., April 27, 1960.

Hon. CHET HOLIFIELD,
Chairman, Subcommittee on Radiation,
Joint Committee on Atomic Energy,
The Capitol, Washington, D.C.

DEAR MR. HOLIFIELD: Thank you very much for your invitation given to me twice, once directly and once through Dr. Inglis to set down additional thoughts for the record. I am very happy to make use of this invitation.

Enclosed I am sending you some of my additional thoughts; some of them are just clarifications of what I said during the hearings, and others are on related subjects. Obviously, this is such a big subject that it is very difficult to be complete in one discussion.

I hope you will be able to insert these additional pages into the record of your hearings.

Some of the political and nontechnical thoughts which I have in this connection I have put down in a talk which I gave last Monday to the Philosophical Society of Washington. I am taking the liberty of sending you a copy of this talk under separate cover.

Thank you very much for giving me this opportunity of supplementing the record.

Yours sincerely,

H. A. BETHÉ.

REMARKS OF DR. HANS A. BETHÉ, SUPPLEMENTING TESTIMONY GIVEN ON APRIL 20 AND 21, 1960, BEFORE THE SPECIAL SUBCOMMITTEE ON RADIATION OF THE JOINT COMMITTEE ON ATOMIC ENERGY, SUBMITTED BY LETTER DATED APRIL 27, 1960

1. The system of stations which has been developed at Geneva (about 21 stations in the U.S.S.R., with 100 seismographs each) is adequate to monitor the limited test ban proposed by President Eisenhower on February 11, 1960, namely a ban on underground explosions giving seismic signals equivalent to an earthquake magnitude of 4.75 or greater. It has been estimated that there will be about 100 earthquakes of this magnitude per year in the Soviet Union, and that 60 to 70 of these cannot be identified as earthquakes and could thus be suspected of being explosions. If 20 inspections are permitted per year, this would allow inspection of 30 percent of the unidentified events. In no case could the coverage sink as low as 10 percent, because even if no identification were possible at all, 20 inspections would still cover 20 percent of all seismic events of this magnitude. I believe that even 10 percent inspection would be a very strong deterrent against the Russians' conducting clandestine tests in violation of a treaty. Moreover, I believe that the estimate of 60 to 70 unidentified earthquakes in an average year is very conservative.

2. Dr. Richard Latter has shown that a mere redistribution of stations, keeping their number at 21, would greatly enhance the capability of the Geneva system to identify earthquakes and explosions. A moderate increase of the number of stations to 30 would reduce the number of unidentified earthquakes above magnitude 4.75 to as few as 9 per year. Magnitude 4.75 corresponds to 20 kilotons under standard Nevada conditions. The number of earthquakes in the U.S.S.R. above 5 kilotons equivalent which would escape identification by the Latter system of stations is estimated to be only 36 per year. Thus, with 20 inspections, the Latter system could deter clandestine tests down to well below 5 kilotons under Nevada conditions.

3. Research on seismic detection will greatly improve our ability to distinguish earthquakes from explosions. We have so far used almost exclusively the first motion as a criterion for this distinction. I expect that closer study will show other distinguishing characteristics, associated with the later parts of the wave. It seems to me likely that 2 to 3 years of intensive research would improve our ability of identification by at least a factor of 5 so that then the Latter system of 30 stations could monitor explosions down to 1 kiloton in standard conditions. Already now, the Geneva system of 21 stations can detect and locate (though not identify) explosions down to well below 1 standard kiloton.

4. The capability discussed in paragraph 3 is important for the detection of partially decoupled shots. The Cowboy experiments have shown that one can get partial decoupling in a hole of smaller size than that required for full decoupling. It has been estimated that a hole of 70 cubic meters (about 2,000 cubic feet) per ton of explosive is required to give the full decoupling by a factor of 300. A hole of one-tenth this volume (7 cubic meters per ton of explosive) appears, according to the Cowboy experiments, to give one-tenth the decoupling, i.e., a decoupling factor of about 30. (I differ from Dr. Albert Latter's testimony in this; he has stated that this decoupling is acheived in a hole of one-thirtieth of the standard volume; however, he has privately agreed with my interpretation.) There are existing cavities in salt domes big enough to take a few, let us say 2 to 3, kilotons with full decoupling. If such a cavity were used to explode a bomb 10 times larger, the decoupling would be expected to be 30; i.e., a 30-kiloton bomb exploded in such a cavity will look like 1 kiloton in Nevada tuff. Such an explosion, however, can be detected and located by the agreed Geneva system.

(See telegram from Dr. Albert Latter, dated May 3, 1960, p. 184.)

5. It is technically feasible today to design a system of seismic stations which would detect, locate, and identify fully decoupled explosions of 20 kilotons. With full decoupling, by a factor 300, a 20-kiloton explosion looks like 70 tons in standard Nevada conditions. A net of stations of 200-kilometers (125 miles) spacing would detect and identify such explosions. These stations would be unmanned and automatic, and each station would have only one seismograph. The optimum design of instruments for such stations will still have to be worked out; it may turn out to be better to have somewhat greater spacing but to have an array of, let us say, four seismographs in each station. Estimates of the cost of such stations have been made by a very competent team at the Sandia Corp. in Albuquerque, N. Mex. The cost is about $100,000 per station, including coding of the seismograph information for transmission and other features designed to make the stations essentially tamperproof. The transmission would probably be by cables which might add another 50 to 100 million for the U.S.S.R.

If fully implemented, this system would involve about 600 unmanned stations in the Soviet Union. However, most of the U.S.S.R. is aseismic, and in this area it would be sufficient to detect and locate seismic events: since there are normally no earthquakes, any disturbance would indicate an explosion. I estimate that for this purpose a spacing of 400 kilometers would suffice, and if this number is accepted for the aseismic regions, the total number of unmanned stations in the U.S.S.R. may be reduced to about 200.

A system of unmanned seismic stations, to supplement the present Geneva system, would greatly increase the capacity of the system to distinguish explosions from earthquakes also at higher yield (1 kiloton and more) and would thus greatly reduce the number of onsite inspections required.

6. The unmanned stations should have seismographs which are capable of recording high-frequency waves (about 10 cycles per second), in addition to the medium-frequency waves (of about 3 cycles per second) normally recorded. This is expected to increase the ability of the unmanned stations to record decoupled explosions and the first motion generated by these explosions. Further research on high-frequency recording and on the noise at three frequencies is required.

High-frequency recording will foil most of the proposed attempts at further decoupling (beyond a factor 300) even if these decoupling proposals should otherwise turn out to be feasible, which is contrary to my expectation. This is because the initial pressure spike, due to the impact of the air shock wave on the wall of the cavity, will be recorded just as strongly at high frequency as the steady pressure in the cavity. Only the steady pressure, not the spike, is reduced by such schemes as the distribution of carbon dust in the cavity.

7. There is also a distinct possibility that the high frequencies will be much weaker in earthquakes equivalent to 70 tons than they are in explosions of this size. If this turns out to be true, the identification of those small explosions would be greatly facilitated.

8. A closely spaced net of stations will probably enable the seismologists to determine the depth of the source of the disturbance much more accurately than is now possible. This would be a powerful tool in distinguishing earthquakes from explosions since only few earthquakes occur in the top mile of the earth's crust.

Such a net would probably also make it very difficult to confuse the signal by setting off several nuclear explosions simultaneously at places a few miles from each other, as proposed by Dr. Teller.

9. In spite of the testimony of Mr. Meade, I still maintain that the construction of a large cavity in salt, of 2 million cubic meters volume, is a major undertaking. A violator would have to be afraid that such a construction job would be detected by other means than seismographs, e.g., by photography from satellites.

The disposal of the brine alone seems a formidable problem. The volume of the brine will be many times the volume of the salt and may be estimated at perhaps 100 million barrels.

(*a*) To dump this into an old oilfield requires a major field, and a lot of pipes which would be visible. Moreover, this would greatly limit the geographical areas where big cavities might he suspected to be constructed, and hence would reduce the number of seismograph recordings which would require onsite inspection to very few.

(*b*) Mr. Meade suggested dumping the brine directly into the sea. I suppose this would be the Caspian Sea. This restricts construction of cavities to the shores of the Caspian, again a very limited geographical region.

(*c*) If the cavity were constructed elsewhere, it would probably be necessary to dump the salt into a river. If this were done in one of the very large rivers in North America—the Ohio, the Columbia or the St. Lawrence—it would at least double their natural salt content over a period of 2 years. Chemical analysis of river water would be a very simple safeguard against digging of salt cavities.

10. The construction of large cavities in limestone, as proposed by Dr. Teller, seems far more difficult than in salt. I discussed this problem with some chemists. The limestone would first have to be heated to about 1,000° C. and decomposed, at considerable expense in power. Then it would have to be cooled to room temperature, at considerable expense of cooling water, in order to have the lime dissolve. More important is the fact that lime is almost insoluble in water: it would require the entire water supply for New York City for over a year to dissolve the lime in a cavity of the size discussed by Mr. Meade. Direct mining operation, at great expense, would probably be more feasible. It is also questionable whether the walls of a limestone cavity would have the desirable elastic properties of a salt cavity.

11. It would be most suspicious if two signals were recorded by the detection stations which originate from the same source. It would therefore be very dangerous for a violator to use the same location for two or more tests. This goes especially for decoupled tests in large cavities if they can be detected by a system of unmanned stations. This greatly adds to complication, cost, and time required for testing.

12. Even if the Russians wanted to violate the treaty (which I don't believe) and even if they wanted particularly to develop small weapons (which I also don't believe) the restrictions imposed by any detection system, even a very imperfect one, would greatly impede their progress and slow it down to a snail's pace. However, if testing were resumed below magnitude 4.75, they could operate openly, easily and quickly and could soon catch up to our present position in tactical weapons. On the other hand, we already have tactical weapons of many yields and sizes, and it would be difficult (though not entirely impossible) for us to make further progress of real military significance.

13. I do not agree with Dr. Brown's statement that tests at 1 kiloton would permit us or the Russians to develop efficient higher yield tactical weapons without further tests. The minimum yield required for this purpose is more likely to be 3 to 5 kilotons. This is also the opinion of the Los Alamos Laboratory.

(The following telegram dated May 3, 1960, from Dr. Albert L. Latter, is hereby made a part of the record:)

RAND CORP.,
Santa Monica, Calif., May 3, 1960.

To: Congress of the United States, Joint Committee on Atomic Energy (room F–88, Capitol).
Attention: Colonel Lunger.
From: Albert L. Latter.

The Cowboy experiments were conducted with chemical explosions, and the results must be interpreted to provide information on the decoupling of nuclear explosions. The interpretation is difficult because the pressure exerted on the cavity wall is quite different in the two cases. For the same yield chemical and nuclear explosions, the average pressure is more or less the same, but the transient impulse is far more severe for the chemical explosion. How one interprets the

Cowboy results depends on whether he thinks it is the transient or the average pressure in the cavity which is the important parameter for determining the decoupling factor. My own view, and the one on which I based some statements in my testimony, is that the transient pressure is more important. Professor Bethé's view seems to be the opposite.

Professor Bethé and I discussed this matter very briefly at the hearing, just prior to his testifying, and apparently he understood me to say that I thought his interpretation is more correct. Actually what I said, or in any case meant to say, was that I agreed that his interpretation is a permissible one. However, I pointed out that on his interpretation the volume of the hole required to give the full decoupling factor of 300 is only 35 cubic meters per ton of yield instead of 70 cubic meters per ton. The reason for this change is that the value of 70 cubic meters per ton, which we presented in Geneva to the Technical Working Group II, was based on the view that the transient pressure is the important parameter. If we revise our view and say that the average pressure is the important parameter, then the corresponding volume for the full decoupling factor is only 35 cubic meters per ton.

The following table might be helpful in distinguishing the two possible interpretations of the Cowboy experiments. The table gives the volume of the cavity in cubic meters per ton of yield required to give a decoupling factor of 300 or 30 according first to Bethé's view and then to mine.

	Decoupling factor	
	300	30
Bethé, average pressure	35	7
Latter, transient pressure	70	2.3

Which of these interpretations is the correct one can, of course, be decided only by decoupling experiments with nuclear explosions. However, in considering the small differences between the two interpretations, we should not lose sight of the really important consequence of the Cowboy experiments; namely, that a decoupling factor of 30 can be obtained with a hole which is much smaller than 70 cubic meters per ton.

ADDITIONAL REMARKS SUBMITTED BY DR. BETHÉ, BY LETTER DATED APRIL 29, 1960

(*a*) The Geneva System of 21 stations in Russia is adequate to monitor the limited test ban proposed by President Eisenhower on February 11. This proposal is to stop by treaty only those underground explosions which give a seismic signal of magnitude 4.75 or greater. This limited ban could be adequately monitored with 20 onsite inspections per year. This limited test ban treaty is the present basis of the negotiations at Geneva.

(*b*) According to Mr. Meade's testimony, it would take 2½ years to construct a cavity in salt to fully decouple a 30-kiloton explosion. If, simultaneously with the limited test ban, a moratorium on smaller tests is declared, to run 2 to 3 years, the Russians could not reap the fruits of such construction during the moratorium.[1]

In this connection it should be pointed out that the Russian scientists in the Technical Working Group II at Geneva were greatly surprised by the theory of the big hole and announced one day that they would demolish this theory. Their attempt to do so was proved incorrect in subsequent theoretical arguments by Dr. Albert Latter and myself. Even if the Russians want to construct such a cavity, which I do not believe, they could not well have started to do so until recently.

(*c*) If a natural cavity, suitable for 2 kilotons, exists in Russia and if it were used to test a 20-kiloton weapon, I already testified that the resulting signal could probably be detected and located by the Geneva net but not distinguished from an earthquake. In this connection it is fortunate that salt dome regions are in general aseismic so that any signal from such a region would be suspicious.[2]

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[1] See testimony of Mr. Meade, p. 156.

[2] See also Mr. Meade's statement, p. 150, for information on Russian salt dome regions.

(Additional remarks submitted by Dr. Bethé, by letter dated May 23, 1960, follow:)

ITHACA, N.Y., *May 23, 1960.*

Mr. JAMES T. RAMEY,
Executive Director,
Joint Committee on Atomic Energy,
Congress of the United States,
Washington, D.C.

DEAR MR. RAMEY: With reference to your letter of May 17, the telegram by Dr. Latter represents the situation quite clearly. However, I believe that a third interpretation is possible and perhaps likely. There may be *some* inelastic behavior and hence some loss in decoupling factor when the *transient* pressure exceeds the inelastic limit and then a much larger loss of decoupling when the static pressure exceeds the overburden. I should therefore like to use Dr. Latter's criterion of transient pressures for the full decoupling factor of 300, which would require 70 cubic meters per ton in this case, and then use my criterion of average pressure for decoupling factor 30 which gives 7 cubic meters per ton for this decoupling factor.

In the case of the smaller decoupling factor of 30, I strongly believe that my interpretation is correct because once the static pressure exceeds the overburden, there must be strong forces leading to inelastic deformation, The transient pressure can in this case only be of relatively minor importance; it may set inelastic deformation in motion but this deformation keeps going because the static pressure is too large. I do not feel so strongly about the criterion for the full decoupling factor of 300; this may be either Latter's or my criterion or something in between. It is certainly not clear how much the decoupling factor would be reduced from 300 if Latter's criterion of 70 cubic meters per ton is violated but my criterion of 35 cubic meters per ton is still fulfilled.

I agree with Dr. Latter's conclusion that partial decoupling is possible, and that this was the most surprising and therefore most interesting result of the Cowboy experiments.

Yours sincerely,

HANS A. BETHÉ.

(Whereupon, at 5:20 p.m., Wednesday, April 20, 1960, the hearing was recessed, to reconvene at 10 a.m., Thursday, April 21, 1960.)

TECHNICAL ASPECTS OF DETECTION AND INSPECTION CONTROLS OF A NUCLEAR WEAPONS TEST BAN

THURSDAY, APRIL 21, 1960

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION,
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittees met, pursuant to recess, at 10 a.m. in room 312, Senate Office Building, Representative Chet Holifield (chairman of the Special Subcommittee on Radiation) presiding.

Present: Representatives Holifield, Durham, Price, Van Zandt, Hosmer, and Bates; Senators Hickenlooper and Bennett.

Also present: James T. Ramey, executive director; John T. Conway, assistant director, George F. Murphy, Jr., professional staff member, and Richard T. Lunger, staff consultant, Joint Committee on Atomic Energy.

Representative HOLIFIELD. The committee will be in order.

The Special Subcommittee on Radiation and the Subcommittee on Research and Development meet this morning in the third day of public hearings on the technical problems of a nuclear weapons test ban and the necessary detection techniques and inspection controls.

We have attempted to follow as closely as possible the proposed outline which we announced at the commencement of our hearings. We have taken longer than originally expected to cover each of the special topics listed in our outline.

I attribute this to the extreme interest of our members in these topics, and the capabilities of our witnesses to augment their written statements with oral answers to the questions from the members. I believe that this is good. I believe that it is important that we discuss these very vital matters in as great detail as possible so that we may fully understand the facts.

While this has been of extreme interest and great assistance to the members of this committee, I realize the extension of the hearings may be causing hardship to some of our witnesses. The Chair realizes that the scientific and technical experts who are appearing before it are busy men and may have other commitments.

Accordingly, if any of our scheduled witnesses will be seriously inconvenienced by not being called to testify at the time and date originally planned, the committee will accept their written statement for the record.

As announced yesterday, we will begin this morning with a panel discussion of the capabilities of monitoring underground nuclear tests during a moratorium. If members of the panel will come forward and take seats at the witness table, we will begin by each identifying himself by name for the record.

55625—60—pt. 1——13

For the record, I will announce that the members of the panel are:

Dr. Roland Beers, seismic consultant, Troy, N.Y.

Dr. Hans Bethé, Physics Department, Cornell University, Ithaca, N.Y.

Dr. Harold Brown, Lawrence Radiation Laboratory, Livermore, Calif.

Dr. Dean S. Carder, Chief Seismologist, U.S. Coast and Geodetic Survey, Washington, D.C.

Dr. Alvin C. Graves, Los Alamos Scientific Laboratory, Los Alamos, N. Mex.

Dr. Richard Latter, Rand Corp., Santa Monica, Calif.

Dr. Jack E. Oliver, Lamont Geological Observatory, Columbia University, New York, N.Y.

Dr. Jay Orear, Physics Department, Cornell University, Ithaca, N.Y.

Dr. Richard E. Roberts, Carnegie Institution, Washington, D.C.

Dr. Carl F. Romney, Headquarters, USAF, Air Force Technical Applications Center, Washington, D.C.

Dr. Edward Teller, Lawrence Radiation Laboratory, Livermore, Calif.

Dr. Harold Urey, University of California, Scripps Institute of Oceanography, La Jolla, Calif.

Dr. Roberts, we appreciate your appearance here today. Would you like to lead off on a statement of what you think we need to improve our detection capability?

PANEL DISCUSSION ON THE CAPABILITIES OF MONITORING UNDERGROUND NUCLEAR TESTS DURING A MORATORIUM

Dr. ROBERTS. Thank you, Mr. Holifield.

I am an amateur seismologist. I have been closely associated with Dr. Tuve and Dr. Tatel in their seismology program. Those men started in 1947 to do explosion seismology. They worked perhaps half time at it for the last 10 or 12 years. During this time they have done a great deal of the work in explosion seismology.

I noticed in Wollard's paper here (Journal of Geophysical Research) that about 40 percent of the data comes from the Carnegie Institution. That is perhaps a sad commentary on the amount of work that has been done in this country when 40 percent comes from two men half time.[1]

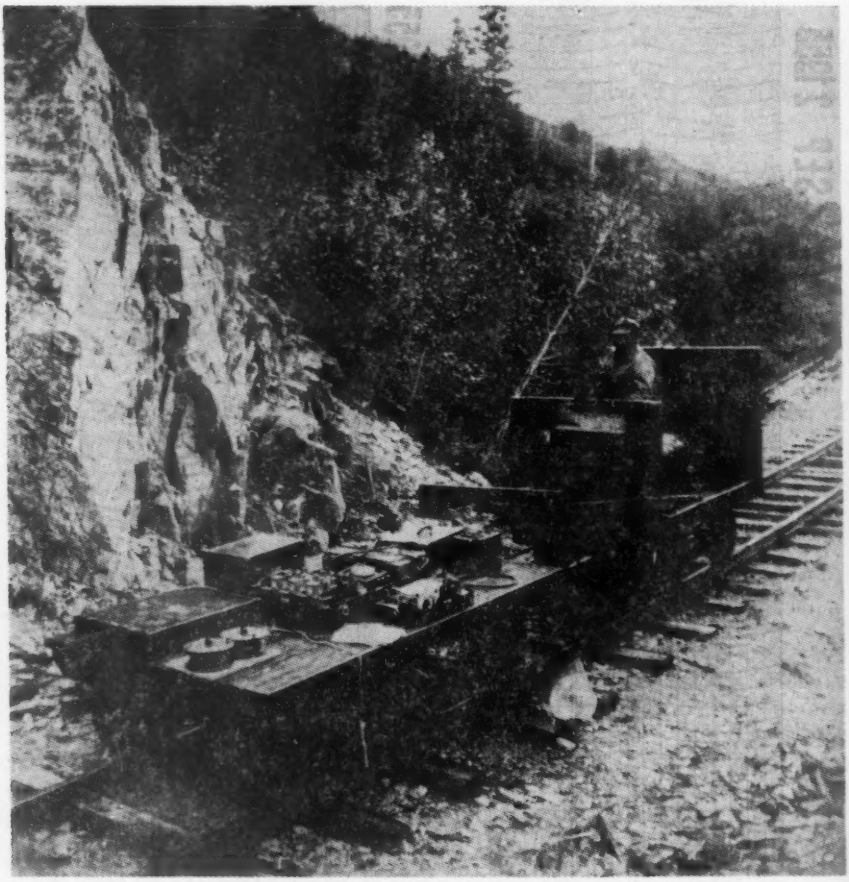
In the course of that time they have acquired a great deal of experience in the seismic experiments, coming from blasts of various sizes. I want to put into the record some of the typcial signals observed and I have handed in this particular one. It shows the seismic signal observed from a 1-ton explosion at 200 and 300 kilometers.

[1] Refer to Beckner Panel Report, app. 5, p. 643, and Seismic Research and Development Report, submitted by Department of Defense, attachment 5, p. 431.

SEISMIC RECORDS RECEIVED WITH PORTABLE FIELD EQUIPMENT, 208 AND 298 KILOMETERS FROM EXPLOSIONS OF 2,400 POUNDS

Submitted by Dr. R. B. Roberts

This is a very nice signal. This is sufficiently above the noise level to be very useful in the detection scheme. Based on this type of experience with equipment which is relatively simple, I would like to show this picture of the equipment.



SEISMIC OBSERVING EQUIPMENT ON A "CASEY JONES" FLAT CAR OF THE YUKON-WHITE PASS RAILROAD BETWEEN SKAGWAY, ALASKA, AND CARCROSS, YUKON TERRITORY

This photograph shows a typical set-up for recording seismic waves from explosions for crustal exploration. In these studies of the earth's crust observations were made over all the continental United States, northwest Canada, and Alaska. At this site, chosen because there were no highways traversing the mountain chain in this area, recordings of shots being fired under water by the U.S. Coast Guard cutter *Sedge* near Skagway, about 40 miles distant, were made.

You can see it is not very elaborate. There it is set up on a handcar on the railroad. It is very easy to detect explosions out to about 300 kilometers—explosions of the order of 1 ton when shot in water and somewhat larger when shot in mining operations.

Explorations have been carried out all over this country, in California, Washington, Arizona, Minnesota, Alaska, and in Peru. During these summer expeditions, they would enroll their children and friends to man the stations.

This particular record was taken by Dr. Tuve's son who had just graduated from college and was on his honeymoon. So it is not an operation that requires highly trained people.

Representative HOLIFIELD. Is that a part-time operation?

Dr. ROBERTS. We have the belief that permanent stations could do much better than these field operations. In these typical field operations you have a station wagon with your seismic equipment loaded in the back, drive up along the road to about the region where you want to make your recording, set up the seismometers on the first reasonable looking rock and take the record, pack up, and drive away 15 minutes later.

We believe that permanent stations equipped with modest arrays could get much better results than these.

One thing that has been learned during these operations is that it is necessary to find quiet sites. You find quiet sites on bedrock or in places where there is no bedrock it is also possible to find quiet sites by drilling wells down to solid rock. This is necessary to get away from surface noise due to wind blowing on trees, and so forth.

Taking these numbers, that 1 ton can be detected out to 400 kilometers, giving a good signal at 300 kilometers and quite reliably at 300 kilometers.

Representative HOLIFIELD. You are speaking of the tamped shots and not the decoupled shots?

Dr. ROBERTS. These are explosions of four 600-pound depth charges, a total of 1 ton.

Representative HOLIFIELD. Four depth charges?

Dr. ROBERTS. Four depth charges tied together and shot off.

Representative HOLIFIELD. Are you talking about them being exploded in the hole in the ground or contiguous to the earth?

Dr. ROBERTS. This is under water. That is the simplest and easiest way to get a reproducible shot.

Representative HOLIFIELD. Of course, that is a nondecoupled shot.

Dr. ROBERTS. That is right. In fact, it is very well coupled. With this experience of being able to detect 1 ton shots, we felt that a 400-kilometer net would be sufficient. Of course, Dr. Bethé's closer spacing would be even better.

Representative HOSMER. May I ask, then, is this the shortwave detection machine or medium wave?

Dr. ROBERTS. These are most sensitive at about five cycles and actually the signals you observe are in the range of six to eight cycles.

Representative HOSMER. Then it is akin to Dr. Bethé's machine.

Dr. ROBERTS. It is close to Dr. Bethé's machine.

Representative HOLIFIELD. It is a low-wave rather than high-frequency type instrument.

Dr. ROBERTS. No. It is a high frequency. We felt that we could detect 1 ton under favorable circumstances, but allowing for the difference between water shots and quarry shots we thought it was much safer to use 10 tons as our detectable figure and we felt that we had another margin of safety because these are single seismos and much better results could be obtained with an array.

So I think this 400-kilometer network, while not as good as the one Dr. Bethé proposed, would be adequate to detect the order of 10 tons. It would also be sufficient to give a highly precise location of the event. With the long distance arrays, the precision is not at all good,

but these close-in stations can detect the first arrival to the order of one-tenth of a second, which corresponds perhaps to 1 kilometer.

So the precision of location is tremendously improved, and there is not a vast area which needs to be inspected in an on site inspection. It is a rather precise location, more or less like a square mile.

I think that is a tremendous advantage both in acceptability and in the usefulness of onsite inspection.

Representative HOLIFIELD. How many stations would this require in the Soviet Union?

Dr. ROBERTS. This would require 125, roughly.

Representative HOLIFIELD. As against the 21 that has been discussed?

Dr. ROBERTS. That is right. I think that it should be emphasized that it is really not a technical problem to detect things. You surely can detect with a close enough network. It is more of a political problem of how many stations are acceptable. Technically we can detect. Politically we may not be able to get acceptance of a sufficient number of stations.

Representative HOLIFIELD. Of course, if you do not get political acceptance of the required number you cut the ground out from under your whole theory of detection, do you not?

Dr. ROBERTS. Of course, a network that is not accepted is of no use whatsoever.

Representative HOLIFIELD. That is right. You ask for 125 stations, and I believe Dr. Bethé asked for 600 of this type in his testimony yesterday. The negotiators have discussed a matter of 20 or 21, which has not been accepted as far as I know by the Soviets.

Dr. ROBERTS. I thought the 21 was accepted.

Representative HOLIFIELD. What about that: Is there any member of the panel that would speak on that subject?

Dr. LATTER. The Soviets have accepted the numbers in the experts' report which referred to 37 stations in Asia. They have not accepted any specific number in the Soviet Union.

Representative HOLIFIELD. Thirty-seven in Asia?

Dr. LATTER. That is correct.

Representative HOLIFIELD. Where does the 21 come from? Is that a discussed assumption?

Dr. LATTER. That is an assumption based on the system requirements in the experts' report of 1,000 kilometers in seismic areas and 1,700 in aseismic areas. You would end up with a number like 21.

Representative HOLIFIELD. Dr Latter, did you testify to a different number on that, around 30 or 40?

Dr. LATTER. I mentioned 21 as the basic Geneva system consistent with the experts' report. I then talked about adding additional stations to that basic net.

Representative HOLIFIELD. How many would that be?

Dr. LATTER. I talked about adding an additional 9 stations, making a total of 30 stations in the Soviet Union.

Representative HOSMER. But that is on the medium-wave system.

Dr. LATTER. That is on the medium-wave system.

Representative HOSMER. I did not understand that from Dr. Brown's testimony. He had moved these spacings up from Dr. Bethé's 300 to a 400-kilometer spacing. But I do not understand how you can bring the number of stations down from 600 to around 100.

Dr. ROBERTS. Dr. Bethé proposed that 200-kilometer spacing be used and the one we calculated was 400 kilometers, so that makes 4 times less stations.

Representative HOSMER. That makes it 150.

Dr. ROBERTS. Yes.

Representative HOSMER. Plus the 21 medium-wave stations, or 30, as Dr. Latter was suggesting.

Dr. ROBERTS. This network was proposed on the basis of a lot of experience in detecting seismic explosions in quarries, and I was pleased to find that Dr. Latter calculated from his data what he would expect and it came out fairly close. There is no great disagreement. He said 18 tons. We had 10 tons. I think our experience has been more fortunate than his in getting good sites with low-noise level. The noise level that we commonly find is around 1 angstrom in a good site and we run at high sensitivity. This is based on a considerable degree of experience. It is not a calculated network based on extrapolation.

Senator HICKENLOOPER. Dr. Roberts, you keep talking about detection. Is there not a difference between detection and identification?

Dr. ROBERTS. Yes; there certainly is. By detection you mean that you get a noticeable signal; your timing is good enough to locate that event with precision. Then it is a considerable problem to know whether that is an earthquake or an explosion.

One thing that helps is that most of the earthquakes occur in rather limited areas so that most of the 19,000 events which Dr. Latter mentioned yesterday would occur in those seismic areas. Fortunately, I believe that the salt domes occur in sedimentary rocks where the earthquakes occur in the mountainous regions, so there would not be very much overlap between the possibility of a salt dome decoupled explosion and the natural earthquake regions.

So of the 19,000 unidentified events, most of them would occur in the mountainous regions and there would be many fewer expected in regions which might also have salt domes. There are a number of the features of the seismogram which may with further research be used to identify earthquakes.

I could not at present say what features are reliable indications of earthquakes. I do not think anybody can.

Representative HOLIFIELD. In other words, there is no way of detecting at this time between a nuclear explosion and an earthquake as far as identification is concerned.

Dr. ROBERTS. Not at this time, but I think there are many promising leads that could be followed up.

Representative HOLIFIELD. You showed us pictures here which look like rather crude or primary types of instruments. Would you say that our present research and development program is adequate or not?

Dr. ROBERTS. I would say it was not.

Representative HOLIFIELD. By a factor of how much?

Dr. ROBERTS. I think when two men have done a large share of the country's work for 10 years, that shows that the research program is primitive.

Representative HOLIFIELD. Is there general consensus of opinion that the research and development in this field is lacking and that a vigorous and aggressive program should be initiated?

I conclude from the nods of the witnesses that they are in agreement on this. If there is anyone in disagreement, let him speak up at this time.

Representative HOSMER. Mr. Chairman, I think when we say whether it is adequate or not we ought to define what the test is. Are you speaking, Dr. Brown, of a program that is adequate to develop a system that does not now exist that would be adequate to produce a high probability of detecting cheating?

Dr. ROBERTS. I would say that the program as it is now conducted is perhaps adequate for basic seismology. There is no tremendous hurry about it. But for the last 5 years, at least, we have known that seismic detection would be an important element in the nuclear test ban and there has not been a corresponding acceleration of the seismic work.

Representative HOSMER. In other words, we do not have the knowledge now to actually be sure about any systems that are under discussion at the present time?

Dr. ROBERTS. We are not sure of any system which can positively identify earthquakes and explosions. I worked on problems which at the outset looked almost impossible, like the proximity fuse and the guided missiles looked very difficult. But this one looks very promising.

With a reasonable amount of work, I should think it would be quite possible to set up operational test networks. Any time an earthquake comes along you go out to the same place and shoot an explosion so that you can calibrate your transmission path and begin to recognize the characteristics of earthquakes as distorted by the transmission medium, more or less like a handwriting expert.

To most people signatures look pretty much the same. A handwriting expert can tell right away what the signature is.

Representative HOSMER. Then this particular graph that was taken by the honeymooning students, having run the thing out over a 24-hour period, produced a number of movements in a line that would be undetermined as to their origin; is that correct?

Dr. ROBERTS. We have instruments like these running continuously, some in Peru where there are earthquakes, some right around here where we detect quarry blasts. Those continuous records show disturbances.

Representative HOSMER. Yes, but I am talking about reading these records. Can they be read so that you know what is indicated, or are there passages in the tape that cannot be interpreted?

Dr. ROBERTS. At present you get a signal like this and you are not sure whether it is an earthquake or an explosion. I have noticed in the records from Peru that their characteristic shape is a small signal out here followed by a considerably larger signal when the shear wave arrives.

In these underwater explosions, the shear wave is almost negligible. In the quarry shot the shear wave is almost the same as the compressional wave. Just by looking at these records you get some idea of a means of identification. That is not good enough. It will take a lot more work. But there is an indication that work will be fruitful.

Mr. Chairman, I have a short prepared paper on seismic detection explosions that I would like to place in the record here.

Representative HOLIFIELD. Without objection we will place it in the record.

(Dr. Roberts' paper follows:)

SEISMIC DETECTION OF EXPLOSIONS

By Richard B. Roberts

Good seismic records have been obtained with portable field equipment located 200–300 kilometers from 2,400-pound explosions. Permanent stations equipped with arrays should give even better records. To obtain these results it is necessary to choose sites which are well coupled to the earth and which have a low noise level. A network of seismic stations located at good sites roughly 400 kilometers apart (closer in regions of high attenuation) should be capable of detecting explosions giving the same seismic signal as these 2,400-pound shots. In addition a 400-kilometer network (after calibration with numerous 1-ton test explosions) should be capable of locating seismic events with precision. This precision makes onsite inspections more acceptable and more valuable. Allowing for less than optimum conditions 10-ton explosions (3 kilotons if decoupled) should be detected and located with considerable certainty by such a network.

Identification of the source of seismic signals is a more difficult problem at present. The signals are repeatable and carry information concerning their source but they are distorted by transmission through a nonhomogenous medium. Accordingly many of the features of the signals are due to the transmission path. Further research, including the operation of a prototype network, is needed to determine which characteristics of the signals might give reliable indications to distinguish whether the source is an earthquake, a surface explosion, a deep explosion, or a decoupled explosion.

When an operational detection network is established, all blasting for industrial or mining operations should be reported in advance and timed. Such blasts would provide valuable calibration data for the system as well as information on the structure of the earth.

Representative HOLIFIELD. I am going to call on Dr. Teller at this time because he has to leave at 11 o'clock. Dr. Teller, do you have any comments to make on Dr. Roberts' or any other witness' testimony since the hearings began in relation to underground tests?

Dr. TELLER. I am a little hesitant to talk. I would be happier if it were possible to talk later because I feel that the few seismologists among us who have worked on this as a full-time occupation should express their views.

Since this is probably really my last chance to talk, I would like to take your kind offer and just say a very few words about the direction in which my worries lie.

The number of events about which Dr. Roberts is talking amounts to many thousands within the Soviet Union. I heard estimates as low as 15,000. I heard estimates as high as 80,000. There is a little experience about the signatures of these little disturbances, about the noisiness in the earth's crust. But we are looking for needles in the haystack.

It will be important to know how many of these earthquakes can be anomalous—producing, for instance, only outward first motions. If you say you want to go back to the place and make an explosion, then again we would have to reproduce thousands of explosions and investigate carefully in each case.

Dr. Roberts has been talking, as I understood at least in part, about explosions in water. He has said, and it is known to all of us, that water is one of the best media in which to couple. Therefore, having clearly observed, a water shot is not as encouraging for other shots as it would at first appear.

Dr. Roberts and Dr. Bethé have made a very excellent point in saying that we have heard here only evidence about decoupling in salt domes. Therefore, we have to worry only about the relatively small number of events which happen to occur in areas in which salt domes exist.

However, apart from salt domes, there are other cavities underground which one might utilize. Furthermore, excavation procedures in other media can be worked out which need not be as expensive as excavation from hard rock. Limestone is a very generally distributed material. I have had occasion to talk with some of my friends in the oil indultry who claim that one can excavate from limestone relatively cheaply by the double process of first heating, burning out the carbon dioxide from the limestone, and converting it to lime, and then washing out the calcium oxide which is soluble in water.

There has been very much less interest in this kind of work than in the salt dome work, and, therefore, estimates of how much it will cost are not readily available.

Whether this will be twice as expensive or three times or five times or perhaps even not more expensive than excavations from salt domes we do not know. But if we are facing people who are willing to go to a little expense, an expense comparable to the many millions of dollars which we have spent on normal testing, it is by no means safe to restrict our attention to areas of salt domes.

I would also like to say with respect to Dr. Roberts' testimony, that I hope you will pay very careful attention to how many inspections will be needed and what areas will have to be inspected in order that the net which Dr. Roberts has been proposing should be operative.

I believe that it will turn out to require several inspections per day in order to convert this system into a reasonably reliable one.

I would now like to say a word or two about the very well thought-out procedure that Dr. Bethé had presented yesterday, which I think is in a little contrast with the one which you have heard about today.

Dr. Bethé's numbers are the same as the numbers we are using. There may be little differences. I am not even sure whether he has been more optimistic or more pessimistic than we are. He has made obviously a very serious effort to be objective about this point.

I would like to make two little comments about his statement. One is that this procedure, if it is implemented with the 600 stations, could indeed be worked out to detect explosions down to 20 kilotons. Below 20 kilotons no capability has been claimed, and if you want to claim for it a capability below 20 kilotons, then I believe the difficulties in that case, too, will have to be considered quite seriously.

Here, therefore, we have a system requiring 600 stations in the Soviet Union alone, and many thousands of stations throughout the world, if you want to make it a really international system, which will then have, according to our present knowledge, a threshold of 20 kilotons.

This threshold can then change in the future downward if, as Dr. Bethé hopes, the methods of detection will be improved. It can change upward if some of those admittedly hypothetical decoupling methods I mentioned yesterday, should work out.

In that case the same network will still be usable, but not down to 20 kilotons, but only down to 50 kilotons. In particular, the number of inspections is, again, a very bothersome question. There are 5,000

events of which Dr. Bethé hopes to eliminate by direct observation 90 percent, and then to eliminate many more by looking only into the areas in which there are salt domes.

It is by no means clear that only the salt dome areas have to be looked at. Furthermore, the system of elimination is based on the idea that the first arrival has a positive phase. I have tried to indicate yesterday that research on several simultaneous explosions will quite possibly—in fact, I would almost say probably—confuse the signal sufficiently so as to make a nuclear explosion look more complicated and uncharacteristic.

I think you should have clearly in mind the difference between an earthquake and a nuclear explosion. The principle of the difference is that the nuclear explosion is something simple, which starts from one point and goes outward. An earthquake, instead, is a relatively complicated effect. By making more than one nuclear explosion, or more than one explosion—for instance, one decoupled nuclear explosion and then another normal explosion or two other normal explosions—you can confuse the picture to an extent where untangling it will become more difficult. Of this I am sure.

Whether untangling it in that case will be possible or not is something which research will have to show, and there I agree with Dr. Roberts and everybody else—such research should indeed be carried out.

I have two more very brief comments, sir, if you will bear with me.

One is that all this amounts to one very simple point which we must clearly recognize. If we try to put in an inspection system, we can do it. The question is only one of the threshold. There is no sane, reasonable inspection system for which it would not be necessary to say that there is an effective threshold, be it the threshold of half a kiloton or 3 kilotons or 20 kilotons or 100 kilotons.

The threshold on the present inspection system is perhaps near 100 kilotons, but more testimony on it may change this number. It is possible, as Dr. Bethé has claimed, to lower this to 20 kilotons. I do not know whether or not this is so. It will depend on the number of inspections.

I think to lower it below 20 kilotons is possible, but will be progressively harder. This question of the number of inspections and the spoofing, the complicated shot, should be carefully estimated.

I would like at the end to make the statement that shots under 20 kilotons—in fact, shots even under 1 kiloton—are exceedingly important for the development of the tactical explosives about which we have been talking yesterday. I think I can say in a completely open fashion that the great majority of our recent shots have been in the small kiloton and the fractional kiloton region, not because of any other reason except the reason of the necessity of developing tactical instruments. We have realized more and more clearly the need for these weapons and the Armed Forces are realizing more and more clearly the same need.

One more little comment. Dr. Roberts has correctly referred to the fact that he has solved difficult problems in his life. I have seen him tackling these problems and he has done a wonderful job, in proximity fuses and in many other fields.

But I have seen many other people equally excellent as he is, and perhaps even more excellent, being licked in trying to do something,

namely, to be heard in a noisy room. The earth's surface happens to be a rather noisy medium in which all kinds of things are going on.

That under good coupling conditions in water, when you know your surrounding, you can produce clear signals is correct. The great number of processes, many, many thousands in the Soviet Union alone, from which we will have to distinguish the real nuclear experiments, this fact alone is something that to me looks very frightening. It will at best require a very great and a very worthwhile scientific effort.

Thank you very much, sir.

Representative HOLIFIELD. You are welcome, sir.

As long as you have mentioned Dr. Bethé, I think I should ask him to reply in your presence, if he cares to, to any technical point which has been raised

Dr. BETHÉ. I want to make only one comment for clarification. Dr. Teller talked about 20 kilotons and 100-kiloton thresholds. I think one should add that some of these figures refer to decoupled shots and some refer to coupled shots.

Dr. TELLER. I have tried to talk about decoupled shots, and it is these decoupled shots we are discussing here. When Dr. Roberts has talked about 1 ton or 10 tons or 20 tons, those were always coupled shots. When Dr. Bethé and I talked we were talking about decoupled shots, which helps to bridge most of this gap and it is a very important one.

Dr. BETHÉ. The system I was discussing in particular is 20 kilotons decoupled.

Representative HOLIFIELD. Were there any other points?

Dr. BETHÉ. I am interested in the limestone suggestion. I want to repeat one thing which I said yesterday, that any attempt to build a large cavity is always a major operation. If you can merely locate the position of the signal, you may know from other evidence whether major operations in this area have been going on.

Representative HOLIFIELD. Is that all?

Dr. BETHÉ. That is all for the moment.

Representative HOLIFIELD. I think we should ask Dr. Oliver now, who is a seismologist, to comment on anything that has been said from the standpoint of your expertise.

Dr. OLIVER. I am a specialist in a slightly different kind of seismology than has been discussed so far. I work mostly in the long-period range of seismic waves. In this field at present we have very little data, so we have been unable to say much about the whole problem from the long-period standpoint.

Representative HOLIFIELD. Will you explain what you mean by the "long-period"?

Dr. OLIVER. By "long-period" waves I mean waves that have periods of the order of 5 seconds or greater, or wavelengths which would be of the order of 30 or 40 or 50 kilometers or greater. We have some data from the nuclear explosions Blanca [1] and Logan recorded on relatively poor instruments. However, with this data we have gotten very encouraging results.

The long-period waves have the advantage of simplicity. They are much more simple and easy to understand than some of the complex waves you have seen on the seismograms today. We now

[1] Underground nuclear explosions of the Hardtack II test series in Nevada in the fall of 1958. The yield of the Blanca shot was 19 kilotons while the Logan shot was 5 kilotons.

have techniques by which we can extend the long-period waves back into the source region and gain information about the source from the long-period wave. Essentially, what we lack is data and testing of this technique, so it does look very promising but that is the present status of that particular technique.

Representative HOLIFIELD. Would you be of the opinion that in order to perfect your data that you should have both nuclear and chemical explosions?

Dr. OLIVER. I think you could test the long-period techniques adequately with only chemical explosions, although they would have to be very large ones, of the order of a few kilotons.

Representative HOLIFIELD. Are there any comments from any of the members on what has been said? Dr. Romney?

Dr. ROMNEY. I would like to rise to Professor Teller's bait for another full-time seismologist to speak a little bit. The point of my comment, I think, is to, in effect, say that I believe the kind of system Professor Bethé talked about is really the one required; that is, with spacing of the order of 200 kilometers rather than 400 kilometers, and the reasons are primarily very similar to what Professor Teller mentioned: These signals recorded at 200 to 300 kilometers are quite correctly stated to be completely adequate for detection.

Representative HOLIFIELD. For detection or for identification?

Dr. ROMNEY. For detection. They are at the same time inadequate for identification. The inspection problem really is involved here. In order that this system be an effective one, you really must identify a very high proportion of these records.

At the present state of our knowledge, using high-frequency information, I believe the only significant technique we have for identification is the measurement of the direction of first motion.

I believe in this particular characteristic the records are not adequate. You can make major improvements in the detectability of the first motion simply by decreasing the spacing slightly in the direction of Dr. Bethé's proposal because of the very rapid increase in the seismic signals as you get closer to the source.

One other point which was mentioned is that this technique will give very precise locations. I think this is true only under very special circumstances. It is really not too pertinent to say that the time can be measured to a tenth of a second and, therefore, the location can be determined within 1 mile. Times on all kinds of records can be measured.

Senator HICKENLOOPER. Are you talking about water explosions?

Dr. ROMNEY. Yes; water explosions. The times on virtually all seismograms that we now have can also be measured to a tenth of a second. This does not mean that the location is good within a mile. The reason for this has nothing to do with the precision with which you can measure time, but, rather, the reason is related to the fact that the travel times themselves vary with position on the earth's surface.

So that although this time is precise to a tenth of a second, there may be a time difference at two locations at the same distance which can exceed a second.

Representative HOLIFIELD. In other words, the speed of the vibration may be different.

Dr. ROMNEY. The speed may be different in different directions. We know many parts of the world where anomalies of 2 seconds exist.

Representative HOSMER. Is that due to the media?

Dr. ROMNEY. Yes, sir. That is due to the kinds of rock and the stratification of the rock.

Senator HICKENLOOPER. Water is pretty much of a constant as far as density is concerned?

Dr. ROMNEY. Yes. I do not think he was talking about a shot observed in water. In this case you could get a very precise location. But for a shot in water observed on land I do not think you can get a very precise location.

Senator HICKENLOOPER. In other words, the earth's crust is composed of very dissimilar elements as you go through it.

Dr. ROMNEY. Yes.

Senator HICKENLOOPER. And the impulse would either be slowed up or speeded up depending on the composition of the area through which it traveled; would it not?

Dr. ROMNEY. That is right. I think you will significantly improve the locations by having more stations and having them closer. But I do not believe you will improve them to 1 mile unless you are willing to go in afterward, after you have observed the event, and fire an explosive charge in that area to determine what the anomalies are.

Senator HICKENLOOPER. You say "closer than 1 mile." Are you still talking about water shots:

Dr. ROMNEY. No, sir. I am talking about shots on land.

Senator HICKENLOOPER. Are you saying, then, Doctor, that it is feasible at this time to locate the point of suspected event anywhere within the vicinity of 1 mile:

Dr. ROMNEY. I think it is not feasible to do that unless you have prior experience with explosions fired in the same region or subsequent experience with explosions fired in the same region.

In other words, if you simply record signals, I do not believe you will know where they originated within a precision anywhere near 1 mile. It will be more like 5 miles unless you have had an explosion previously fired in the same general region or unless you are willing to go in afterward as part of your inspection and fire a shot in that region.

Senator HICKENLOOPER. Within 5 miles, assuming that is a radius, that would be 100 square miles?

Dr. ROMNEY. Seventy-five.

Senator HICKENLOOPER. Somewhere in the circle. I was just taking the square instead of the circle. I have forgotten what factor you apply.

Dr. ROBERTS. I agree with Dr. Romney about the precision. In my written statement I said "after numerous calibration shots."

Dr. ROMNEY. Yes.

Dr. ROBERTS. The other point I want to mention is that I brought this record of a shot in water because it was from the Alaska trip where I was one of the observers. We have about equal experience with quarry shots. I do not think the point whether it is shot in water or in a quarry should be too much stressed here.

Dr. BETHÉ. Excuse me. What is the ratio of the signal from quarry and water?

Dr. ROBERTS. It varies considerably from one shot to another. The quarry shots are often calculated to move earth. There are all different kinds of shots in the quarry. The usual sort of rule-of-thumb factor is that a quarry shot will be five times less effective than the water shot. Some are even less effective.

Representative HOLIFIELD. That is considerable, five times.

Dr. ROBERTS. Yes. We put in a factor of 10 to care care of that.

Senator HICKENLOOPER. Dr. Roberts, the whole basis of the discussion here on detection and identification is not based upon an incident which occurs under ideal conditions and known conditions, but is based upon the determination of someone to conceal it and to confuse.

Dr. ROBERTS. Yes, sir.

Senator HICKENLOOPER. So I am not so sure that we can go too much on a classic set of conditions because anyone that wants to cheat would try his best to fuzz it up some way. So it would not give the answers which might otherwise be given if it were in a known type of earth structure.

Dr. ROBERTS. We have heard decoupling discussed at length, and that it will reduce the amplitude of the signal. So all of these numbers take that into account. The decoupling is uesd to reduce the amplitude.

There is no way to affect the precision of location. The identification is more difficult, certainly, than getting a precise location. But having a precise location is the first step, and knowing what has happened. If it is in some area where there is no possible access, you can rule it out just from that point of view. But knowing the location is a long step toward detection.

Representative HOLIFIELD. Dr. Beers, would you like to comment?

Dr. BEERS. Yes, sir; Mr. Chairman.

I would like to introduce a slightly different note into the discussion, if you will permit me. If you do not mind, I would like to tell you a short story.

This is about an old prospector who had spent all his life going around the country and searching for rich treasure and had managed to save some out of what he had found. Then at the end of what appeared to be an ordinary career for a prospector, suddenly a great bonanza was discovered in Canada.

The base metal deposits of eastern Canada and New Brunswick appeared to be fabulously larger than anybody anticipated. So the prospector decided he would go up and stake out his claim in this area. This prospector was not, such as you might imagine in the gold rush, carrying a pick and shovel. He had tools which were different.

He employed airplanes and helicopters and electronic devices and a great many experienced scientists who helped him in his prospecting work. Over a period of about 2 years he covered about a million square miles along the Atlantic seaboard from Canada to Georgia.

In the midst of this, after he had made many discoveries of anomalies to be evaluated, the base metal market went down very, very low and it was no longer economically feasible for this prospector to go ahead and dig out his ore.

This has a bearing on the problem before us because we are confronted with many problems here, one of which is numbers. This prospector discovered, in exploring a million square miles, something

like 10,000 anomalies that had to be evaluated. This meant that he had to go on the ground with his scientists and with his geophysical equipment and examine these at great expense and great effort and great time.

After the period of examination was completed, he had in hand 100 prospects which were worthy of drilling and seeing if the ore was rich enough to dig out. But, as I said before, in the midst of this whole program, the base metal market made it economically unprofitable.

What was he to do? One day a prospector of the old variety came into his office and laid a piece of ore on his desk. He looked at it with interest because it was a sample of nickel ore which at that time had remained in a stable price market. It was a piece of primary nickel ore which had not been generally known to occur in the United States.

So the prospector redeployed his equipment and people and investigated this deposit of nickel ore. The elements which are comparable to our present problem are these: We have gathering power for a tremendous amount of information in our teleseismic net. In the onsite inspection teams we have also the ability to gather a great deal of additional information.

The point of the story which I wish to make is that in spite of all these scientific efforts, the advent of intelligence by bringing a piece of ore in from a prospector who had not employed all these methods was probably more significant than all of the scientific effort which he had carried out.

It seems to me that in our discussion here we are losing sight of a part of our program, which is the onsite inspection program, the purpose of which is to enhance the significance of questionable anomalies that may be detected by the teleseismic net.

There may not be a complete evaluation of each event at the time it is examined, but the effort of the teams which are working on it to bring it to a successful conclusion may have as much significance as all the other items which we have considered together here.

Senator HICKENLOOPER. Dr. Beers, do I understand the point of your discussion goes back to the old bridge theory that one peek is worth two finesses? So after all is said and done, we can have a lot of scientific data, but the proof of it is the actual onsite proof; is that correct?

Dr. BEERS. Yes. The point I am trying to bring out here is that we should not lose all hope of the capability of the system which has been proposed by the various experts in the field.

Senator HICKENLOOPER. I agree we should not lose hope, but we should not be unduly optimistic about it either, so far as its present ability to solve some of the very pertinent problems in this field is concerned.

Dr. BEERS. Let me be specific. May we consider Professor Bethé's suggestion, for example? May we consider Dr. Romney's suggestion?

It seems to me that these are all pointed to an improvement of capability which, if it is realized, will place us in a quite different position than we stand at the present time. Therefore, the program of research which has been recommended by the many experts would enhance capability above all estimates at the present time.

Representative HOLIFIELD. I would like to dissect your story just a minute and say this: It depends upon the old prospector being allowed to go out and use his pick and shovel and bring in the piece of nickel ore which the scientists did not discover.

We face a very tight problem along that line in obtaining information from behind the Iron Curtain and obtaining access behind the curtain. Therefore, we are dependent, as far as I know—I would say 99.9 percent—upon some type of electromagnetic or seismic detection system, because it is a plainly known position of the Soviet Government that they do not allow people to roam around in their country.

We are going to get onto the onsite inspection procedures. We recognize that the installation of 600 stations and the corresponding personnel and the willingness of the Soviets to allow inspection teams to go out to inspect suspicious events; all of these things are political problems. That is true.

We have no competence to go into the political problem at this time. So, therefore, we are directing our attention to the scientific detection techniques which are possible at the present time and also the desirability of improving them and how they can be improved for the future.

For instance, if Dr. Bethé's request for 600 stations, or Dr. Roberts' request for 125, is denied, and you are only allowed 5 stations, the problem is solved immediately. You have no detection capability. But this is something that we cannot do anything but speculate on because the negotiators have never gotten to the point of decision in this field.

Representative HOSMER. Getting right down to that old prospector and nickel ore, he found it as a matter of luck. Was that not it?

Dr. BEERS. The point I wanted to make is that we have other factors which may be significant.

Representative HOSMER. Essentially you are telling us that we should trust to luck on finding these things.

Dr. BEERS. Not at all. What I am saying is that there are other factors to enhance the capability than those presently considered.

Representative HOSMER. When the old prospector walked in with a piece of nickel ore, into this old fellow's office who had done all the work, how old and nearly dead was the old fellow who had done all the work?

Dr. ROMNEY. I think Dr. Beers should be identified as the old prospector and he looks far from dead.

Representative HOLIFIELD. May I call on Dr. Orear now? Do you have any comments to make at this time, sir, on what has been said, or any other comments you wish to make?

Dr. OREAR. I think maybe I could make two short comments.

In comparing the existing Geneva net of 180 stations with the one proposed by Dr. Bethé, the existing one would give us about 100 unidentified events above magnitude 4.75; whereas Dr. Bethé's would give us about 500 unidentified events down to a signal level 300 times lower.

In other words, Dr. Bethé has given us a system which is 300 times better than the Geneva net we are now thinking of. The question has arisen, this increase in the number of unidentified events is a serious problem.

I would like to make one small point: that perhaps it is not as serious as we think. I say that because this problem of the onsite inspections is mainly a problem of exploring in detail a certain number of square miles of the Soviet Union.

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If you figure it out in terms of the number of square miles that have to be looked at, it is about the same. If calibration shots are used in setting up Dr. Bethé's net, the area of uncertainty can be reduced from 100 square miles to about 1 square mile.

So in terms of access to the Soviet Union that we are asking for, we are asking for roughly the same number of square miles. We would like to have more on site inspections, but over a much smaller area for each on site inspection. That is my first point.

Then I have a second point. The comment that Dr. Teller made when he said shots below 1 kiloton are exceedingly important. This is, of course, a political judgment which is outside my field.

Representative HOLIFIELD. Why would that be political? Dr. Teller is probably one of the outstanding weapons experts in the United States. When he said that a 1 kiloton shot would be valuable from the standpoint of developing tactical weapons, why do you say that is political?

Dr. OREAR. That is not political. The political judgment is judging how important it is to improve our tactical devices below 1 kiloton.

Representative HOLIFIELD. That is a different problem.

Dr. OREAR. None of us here can make that judgment.

Representative HOLIFIELD. We are not going into that problem in these hearings. This may be his opinion. I took his statement to mean that from a technical standpoint a kiloton explosion was important in developing weapons.

I know well his position, but I did not take it to mean that he was saying politically that we should have this. He was speaking from a technical standpoint as I understood it.

Representative HOSMER. Dr. Orear, the high-yield technology is better known than the low-yield technology in these devices, and as a consequence, it would be reasonable to expect that the testing area would be in the low yield and not the high yield; is that it?

Dr. OREAR. Yes. I am just raising this political question, which we cannot answer, just what would be the value if the Russians could secretly test that low?

Representative HOSMER. Insofar as they are concerned, maybe they do not care about anything but a great big, dirty, clobbering device. They would have no reason to do any more testing than they have done.

Dr. OREAR. Dr. Teller's comment that there will always be a threshold, I think, is a good one. This means no matter how the detection improves or how the concealment improves, there will always be a threshold. It will probably be somewhere in the kiloton region.

I think in talking with Dr. Teller and reading what he has said, no matter where this threshold is, whether it is at 1 kiloton or 100 kilotons, or even a fraction of a kiloton, he would always be opposed to a test ban treaty.

Representative HOLIFIELD. That does not enter into the problem. We know he would be opposed to it. The problem we are considering is can a test of a certain number of kilotons be held without detection or identification? It is a technical question.

Dr. OREAR. Yes. This is a technical answer we can all give you immediately. We can say there will always be some threshold. There will always be some small yield that we cannot detect technically.

Representative HOLIFIELD. The weight of the testimony up to date certainly does establish that point.

Were there any other points you had?

Dr. OREAR. Not now; no. Thank you.

Representative HOLIFIELD. Dr. Carder, do you have anything to say on the comments that have been made?

Dr. CARDER. I agree with most of them. In the first place, you cannot under present capabilities identify whether or not most of these seismic events are earthquakes or nuclear or manmade explosions.

I have looked at quite a few records. At first I thought that one could differentiate between an earthquake and an explosion, especially if the seismograph was within 200 or 300 kilometers. I thought by looking at the records from these explosions that they had a different appearance from the records of most earthquakes.

Representative HOLIFIELD. These are explosions in the air?

Dr. CARDER. These are explosions in the air. But as soon as I saw records from underground explosions, I had to change my mind.

Representative HOLIFIELD. What is the difference between a nuclear explosion underground and a high-order chemical explosion in the seismic reading?

Dr. CARDER. As far as I am concerned, there is none. There is no way of identifying the two.

Representative HOLIFIELD. We have had testimony that there is an upward motion of the seismographic needle in one instance and an initial downward movement in the other if you catch it on the initial movement.

Dr. CARDER. Are you referring to a nuclear explosion and high explosive?

Representative HOLIFIELD. Yes.

Dr. CARDER. In the case of a nuclear explosion, on account of the speed of the thing, the reflection may give a downward flip. Is that what you mean?

Representative HOLIFIELD. That is what I mean.

Dr. CARDER. We do have one case from one of the Nevada underground nuclear explosions, where we looked at the record from about 250 kilometers away. The first motion was definitely down on that one.

Representative HOLIFIELD. You say one. We have had six tests out there.

Dr. CARDER. Yes. The rest were up.

Representative HOLIFIELD. The rest were up?

Dr. CARDER. The rest of them were up.

Representative HOLIFIELD. Is that so? Is this the general consensus of opinion? If it is, it is new to me.

Dr. BROWN. The close ones were up.

Dr. LATTER. At the near-in stations.

Representative HOLIFIELD. How about your stations at identical distances? Further away I mean. Are they always down further away?

Dr. BROWN. No, sir. Some of them were up and some were down.

Representative HOLIFIELD. Then you have no clear signature on either type of explosion, do you?

Dr. BROWN. If you get close enough in, I think the evidence is that the first motion will be upward, with a few anomalies.

Dr. CARDER. Yes.

Representative HOLIFIELD. How close is that distance?

Dr. BROWN. It depends on the yield of the explosion, but generally within a 700-kilometer range, a 20- or even a 5-kiloton explosion tends to give an upward motion.

Representative HOLIFIELD. And therefore, a downward flip.

Dr. BROWN. No; upward.

Dr. CARDER. That is the case mostly. There is one out of six that was the opposite in the case of a station about 250 kilometers away.

Representative HOLIFIELD. Would that occur on a multitude of stations? If you had a hundred-seismograph panel, would that characteristic be true of all of them or was this just due to a particular incident?

Dr. CARDER. I think it is due to the particular local condition at the instrument.

Representative HOSMER. A geological anomaly that gave you a bounce?

Dr. CARDER. Yes; a geological anomaly. I would like to make another statement here in this regard.

I have examined close-in records from the Hardtack II series in Nevada and found that about a quarter second after the initial arrival that there was a strong, oppositely directed movement. If this initial arrival were not strong enough to show above the background, this strong, later arrival would confuse the record.

I am referring not to the high-frequency waves, but the ones which have a frequency of about one a second, or a period of 1 second.

Representative HOLIFIELD. Were these the type that were used at the Blanca shot?

Dr. CARDER. Yes.

Representative HOLIFIELD. Is this the chart you are referring to where the first substantial movement was—I do not know whether you would call this blip the first movement.

Dr. CARDER. I would call that the first movement.

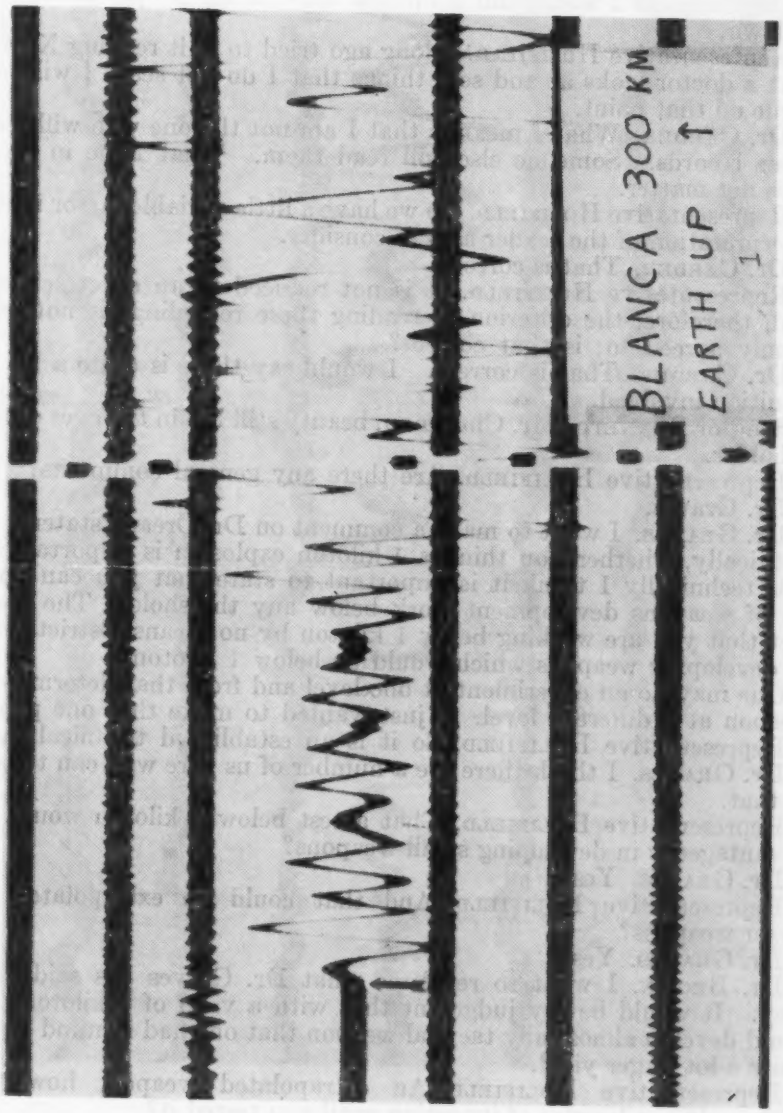
Representative HOLIFIELD. That is up?

Dr. CARDER. That is up.

Representative HOLIFIELD. At 300 kilometers.*

Dr. CARDER. Yes.

*Seismogram—"Blanca 300 km," p. 207.


BLANCA 300 KM
EARTH UP
1

Representative HOLIFIELD. Then at 2,200 kilometers the same shot shows a down motion.*

Dr. CARDER. To most people; yes. In my own case I have looked enough at those things that I would call that "up."

Representative HOLIFIELD. You would call it up?

Dr. CARDER. I would call it up, but most of the people would call it down.

Representative HOLIFIELD. I long ago tried to quit reading X-rays that a doctor looks at and sees things that I do not see. I will not argue on that point.

Dr. CARDER. What I mean is that I am not the one who will read those records. Someone else will read them. What I see in them does not matter.

Representative HOLIFIELD. So we have a little variable factor in the interpretation of the reader also to consider.

Dr. CARDER. That is correct.

Representative HOLIFIELD. It is not reduced to an exact science, and, therefore, the criterion of reading these recordings is not uniformly agreed to; is that correct?

Dr. CARDER. That is correct. I would say there is quite a bit of intuition involved.

Senator BENNETT. Mr. Chairman, beauty still lies in the eyes of the beholder.

Representative HOLIFIELD. Are there any general comments?

Dr. Graves.

Dr. GRAVES. I want to make a comment on Dr. Orear's statement. Politically, whether you think a 1-kiloton explosion is important or not, technically I think it is important to state that you can do a lot of weapons development work below any threshold. The mere fact that you are working below 1 kiloton by no means restricts you to developing weapons which would be below 1 kiloton.

One may do an experiment at one level and from that determine a weapon at a different level. I just wanted to make that one point.

Representative HOLIFIELD. So it is an established technical fact.

Dr. GRAVES. I think there are a number of us here who can testify to that.

Representative HOLIFIELD. That a test below 1 kiloton would be advantageous in developing small weapons?

Dr. GRAVES. Yes.

Representative HOLIFIELD. And that could be extrapolated to larger weapons?

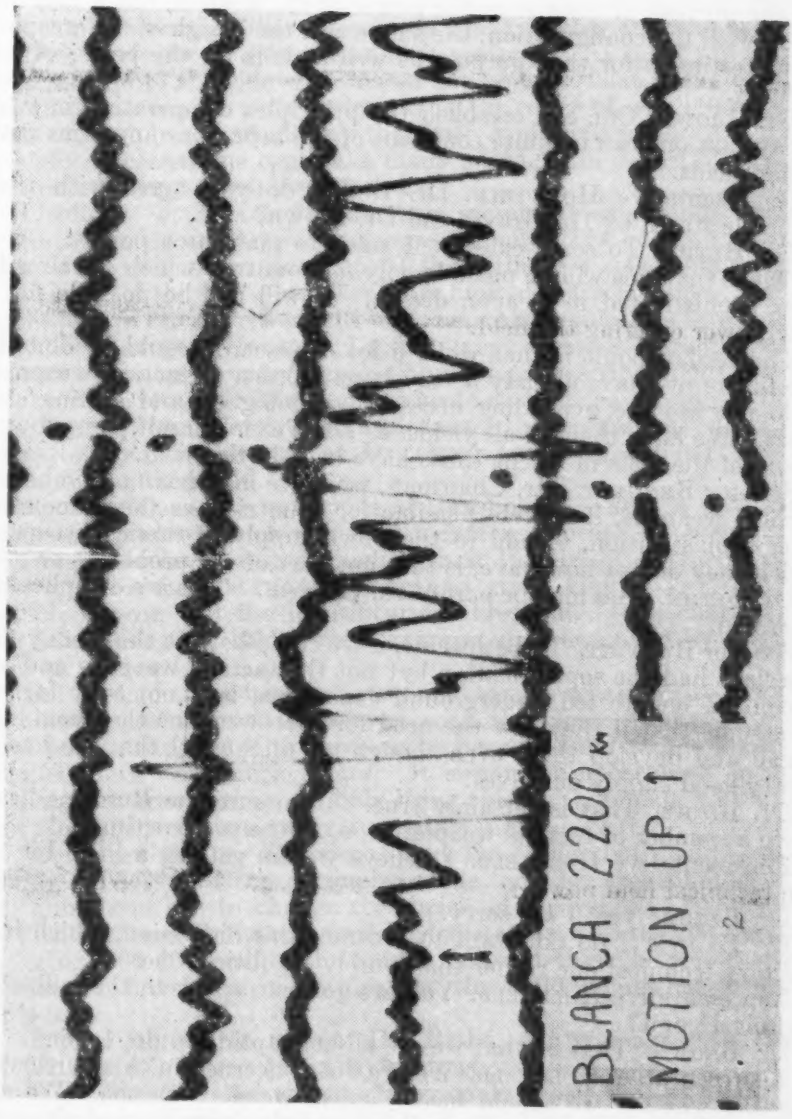
Dr. GRAVES. Yes.

Dr. BROWN. I want to reinforce what Dr. Graves has said, if I may. It would be my judgment that with a yield of 1 kiloton one could develop almost any tactical weapon that one had in mind up to quite a lot larger yield.

Representative HOLIFIELD. An extrapolated weapon, however, you would not be sure of its action until you tested it?

Dr. BROWN. I think one would in almost all cases be completely sure on the basis of tests as low as 1 kiloton, if one is talking about the tactical weapons field.

* Seismogram—"Blanca 2200 km," p. 209.


BLANCA 2200 km
MOTION UP 1
1
2

Mr. RAMEY. You mean going up to say 20 kilotons?

Dr. BROWN. For example. It depends on the design of the weapon, of course.

Representative HOLIFIELD. In other words, the testing is not alone for the purpose of the size or the power of the weapon, but it also has to do with the configuration, the shape and the design of the weapon. Your testing is for that purpose as well as it is for the power of the weapon.

Dr. BROWN. One can establish the principles of operation in such a way that one can be quite confident of the behavior of systems with larger yields.

Representative HOLIFIELD. Dr. Bethé, do you agree with that principle stated by Dr. Graves and Dr. Brown?

Dr. BETHÉ. To some extent. I want to make two points:

One is that this will be possible only for countries which are already very sophisticated in weapon design. It will not be possible for a new power entering the field.

The second point is that while a lot of research could be done in the future, we have already a very large number of tactical weapons. I do not see the overriding necessity of going on and getting still more. We have them of all yields, as Dr. Teller himself pointed out. Many of the tests in recent times have been in this field.

Senator BENNETT. Mr. Chairman, while we may have a number of tactical weapons, if it were possible for countries, as they increased their sophistication, to add to their own supply of tactical weapons which they do not now have, is that not part of our problem?

Dr. BETHÉ. This may be part of our problem. This is a complicated situation.

Senator BENNETT. To be specific, if we could be sure that today the Russians had the sophistication but not the tactical weapons and we permitted undetected underground explosions, is it not very logical to assume that if they saw the need for such weapons they could go ahead and develop them even though we might feel that we had a pretty good supply ourselves?

Dr. BETHÉ. That is certainly true. I am sure the Russians have quite a number of tactical weapons, too, at the present time.

Representative HOLIFIELD. I believe we are getting a little bit off the technical field now.

Dr. BETHÉ. Yes; I am sorry.

Dr. BROWN. I do not want to protract this discussion, which is a military-technical one rather than purely a political one.

Representative HOLIFIELD. You are going to speak to the military-technical point?

Dr. BROWN. That is what we are all attempting to do, I think.

Representative HOLIFIELD. We are not concerned in this particular meeting whether we should have tactical weapons or not. We are concerned with the technical possibilities of obtaining tactical weapons with an explosion which might be concealed, which is a technical question.

Dr. BROWN. That is correct, Mr. Holifield. I think that Senator Bennett has made clear that there are really two questions here only one of which really interests us in the present context.

One can ask the question: "What does it profit each of two countries relatively to have tactical weapons?" We are asking here instead

another question: "What happens if one country cheats and another one does not? What kind of relative advance can the first one make?" One can also ask another question, partly military and partly political, which we are not trying to answer: "How important is such an answer?"

I think there is another comparison that probably ought to be made. I think Professor Bethé referred to it yesterday. I think my opinion does not quite coincide with his on the point. That is the question of what one can do with explosions in the range of, say, 10 or a few tens of kilotons. I would say that with explosions in the 10-, 20-, or 30-kiloton range, one can make many advances in design of strategic weapons of very much larger yield.

I will not make exactly the same statement as I made about 1 kiloton relative to tactical weapons—namely, that one can make all the advances one can think of. I think this is a somewhat different situation. I think one is rather more limited in the advances that one can make in strategic weapons with concealed explosions in the 10- or 20-kiloton range. However, I think a very substantial fraction of the advances that a country developing nuclear weapons might want to make in the strategic field could be made with such explosions. These two figures—one kiloton and 20 kilotons, I think—are fairly significant ones to examine, because as Dr. Orear has pointed out, there will always be a threshold.

Actually there are two kinds of thresholds. There is a detection threshold, below which one can in fact not locate an event at all. There is another threshold higher than the detection threshold for any given system station spacing and given kind of instruments—which we can call the identification threshold. It is characterized by being the yield below which the system is swamped by the number of inspections.

Dr. Orear, I think he may have failed to hear what Dr. Romney said previously, which was that even with the closer spacing one does not reduce the size of the area to be inspected unless one knows the travel times very much better. It is not the uncertainty in establishing the arrival time. I only bring this up to explain why a large number of inspections can very well swamp the system.

One must also, as Dr. Bethé and Dr. Teller both said, consider things in terms of the decoupled shots, and for any amount of decoupling one has to change the threshold accordingly. The system which Professor Bethé recommended yesterday with the 200-kilometer spacing, 600 stations, in the Soviet Union, I think he would characterize as having an identification threshold somewhat below 20 kilotons.

Representative HOLIFIELD. Dr. Bethé, did you agree to that?

Dr. BETHÉ. Yes. About 20 kilotons.

Representative HOLIFIELD. Now, could I ask you at that point, Dr. Brown, do you agree with Dr. Bethé that with his 600 stations in the U.S.S.R. and 5,000 events, that 90 percent could initially be identified as earthquakes, leaving 500 for further identification?

Dr. BROWN. So far as one can tell from these numbers, I think that is probably correct. I would like to point out that this is based on a decoupling factor of 300. The decoupling factors can change.

Some of the methods that have been proposed may not work, although I would like to interpolate here the remark that the experi-

ments which Dr. Penney did were not with graphite, but with aluminum, so they may not be applicable in disproving the usefulness of graphite as a heat absorber.

Some calculations done at my laboratory would indicate that one can get a factor of three increase in decoupling factor merely by stacking a lot of graphite in solid blocks, which are known to exist around the explosive. So it may be possible to get another factor of three in decoupling that way. I think eventually it might be possible to get a factor of 10.

Suppose one wants to keep the identification threshold where Professor Bethé set it for his system of 600 stations in the Soviet Union, and where I think I agree it is for a decoupling factor of 300, somewhere in the neighborhood of 20 kilotons. One would then probably have to increase the number of stations by another factor of three (if the decoupling increases by a factor of three) according to some calculations that Dr. Richard Latter made.

Representative HOLIFIELD. Is that 3 over the 21 or the 600?

Dr. BROWN. 600, although Latter points out that if the increase in decoupling is 10, then the increase in the number of stations will have to be a factor of 10, not 3.

Representative HOLIFIELD. Dr. Latter, will you state your opinion on that?

Dr. RICHARD LATTER. Yes, sir.

Let me make a few remarks on this question of the capability of the 600-station system. In point of fact, if it is possible to identify earthquakes by their rarefactive motions, then Dr. Bethé's system would be an excellent system, even better than I think he suggested it would be.

I would find very few events unidentified at the 20-kiloton decoupled threshold.

Representative HOLIFIELD. You mean it would come down to 20?

Dr. LATTER. Starting with 20 kilotons decoupling by a factor of 300 would bring you down to about 70 tons, which Dr. Bethé was talking about. There is one unfortunate feature about looking at the identification in this manner, of looking for rarefactions from an earthquake.

It is known that some earthquakes produce only compressive first motions just like explosions. It is not known exactly how many such earthquakes occur. One could make a very rough estimate of how many are likely to occur and this might be as much as 10 percent of all the earthquakes observed.

This would mean that if you had 5,000 earthquakes, 500 of them would have only compressive motions and would be suspicious events. This would then correspond to Dr. Bethé's number of 500.

However, if you try to improve the system and add more stations—lower the threshold—you encounter a greater number of earthquakes. I doubt that you will identify all of these earthquakes. I think you will not do better than perhaps 90 or 95 percent identification, due to the residual earthquakes—the 5 or 10 percent of the total number—which have compressive first motions in all directions.

So if you try to improve the system down to, say, 30 tons, where there are about 10,000 or so earthquakes per year in the Soviet Union, you would be faced with about 1,000 unidentified events.

Representative HOLIFIELD. Dr. Brown?

Dr. BROWN. I think to look at a slightly different possible threshold—I think we have been looking at 20 kilotons—suppose we look at 1 kiloton. At 1 kiloton I think it is very likely that one would be able to decouple by a larger factor than 300. It certainly gets easier the smaller the yield gets because these elaborate schemes scale in difficulty with the yield. As the yield gets smaller——

Representative HOLIFIELD. The size of the underground hole would be smaller.

Dr. BROWN. And the amount of graphite or foam that you would put in, whichever of these ideas work. If one assumes a 3,000-fold decoupling of a kiloton explosion, then one is talking about a third of a ton. Suppose one wants to get the same kind of identification that Dr. Bethé was talking about for the 20-kiloton explosion decoupled 300-fold. I made a calculation which indicated there is a factor of 20 increase in the number of stations due to the increase of yield from 20 to 100; there is a further factor of 10 increase due to the decrease in decoupling.

One is talking about a total factor of 200 and that means at least 200 times as many stations, perhaps more if Dr. Latter is right in what he says about the number of stations required going up more rapidly than the yield.

This brings you down to about a 30-kilometer spacing, and this works out to about 120,000 stations, which is a pretty good network. All this means that one can get an arbitrarily good system by having an arbitrarily large number of stations. In fact one can have people at stations 10 feet apart and one could then get the threshold down very low.

Representative HOLIFIELD. Let us not go into that improbable situation. I doubt if the Soviets would like people every 10 feet.

Dr. BROWN. One can take any system one can get accepted (it is not clear what that system is) and calculate the threshold for it. All my previous calculation means is that Dr. Orear's statement that the threshold will not go below 1 kiloton is almost certainly right. It is far above that with the present station spacing and understanding of seismology.

The question is whether the threshold will go up or down. It will go down as you increase the number of stations, improve your seismic detecting apparatus, and learn more about the structure of the earth. It will go up as you learn how to decouple further.

Representative HOLIFIELD. Let me ask you a specific question. The Berkner Panel on seismic improvement predicted that the probable result of several years of vigorous research would be to increase detection capability by a factor of a few, assuming a fixed number of control stations.

Is this the best prediction: I recognize that few is an indeterminate number.

Dr. BROWN. Probably some of the seismologists know the answer better than I, but I will try to answer it anyway. I think the best indication is, there will be an improvement by further work on seismological instruments and other parts of seismology, by at least a factor of a few.

Representative HOLIFIELD. Could you come nearer than the word "few": Could you say by a factor of 3 or 5?

Dr. BROWN. Maybe that means 2 or 3 or 5. I think there is a small chance, but not a zero chance, that by such things as seismometers in deep holes, and so on, one may be able to get quite a bit larger factor than that, 10 or 50 or so. However, in establishing what the threshold will then be, if these efforts succeed, one must then ask how much change occurs in decoupling?

Representative HOLIFIELD. That is what I was going to ask you. If it can be assumed that we will increase our detection capability by a factor of 5, is that a reasonable number?

Dr. BROWN. I think it is reasonable. I would have to defer to other people.

Representative HOLIFIELD. If so, what significance would the factor of 5 have on the factor of 300 on decoupling?

Dr. BROWN. Almost none. That is why Dr. Roberts and Professor Bethé have pointed out that to cope with factors of several hundred in decoupling you must do something quite different that improving the seismic instruments at a given station. You must greatly decrease the net spacing.

Representative HOLIFIELD. Do you agree with that, Dr. Bethé?

Dr. BETHÉ. Yes.

Representative HOLIFIELD. You would have to increase the number of stations in that event?

Dr. BETHÉ. In order to cope with the 300 decoupling you obviously have to increase the number of stations.

Representative HOLIFIELD. If the present seismographs were improved by a factor of 5?

Dr. BETHÉ. Clearly; yes.

Representative HOLIFIELD. Dr. Urey?

Dr. UREY. I think this discussion is very interesting, but I would like to propose a somewhat different question.

We have been talking a great deal about how many stations we need in the Soviet Union. I would like to ask how many we think we need in the United States?

Representative HOLIFIELD. I think some of the preliminary statements anticipated about the same number in the free world as in the Soviet Union. Is that correct, generally speaking, gentlemen?

Dr. UREY. Let me tell you what I am thinking of.

Representative HOLIFIELD. We are thinking in terms of putting them 1,700 kilometers apart.

Dr. UREY. I am thinking that in the very, very improbable event that we could get the President of the United States, this committee and the Atomic Energy Commission, a whole host of engineers and scientists to try to make a test after we had signed a solemn treaty, that it would be exceedingly difficult to hide any test for 6 months. All you need is the New York Times in order to find out what has happened.

Representative HOLIFIELD. You mean in the United States.

Dr. UREY. In the United States.

Representative HOLIFIELD. Or any other democratic country.

Dr. UREY. Now, then, the problem we have in arranging a network with a threshold of this kind is simply this: The threshold finally becomes zero. This depends upon other things than these wiggly curves. I suppose that our inspectors, our people that man our stations, would not be completely isolated from the population.

They would speak the language. They would be able to observe other things than this. Finally there becomes a limit beyond which you cannot make any tests of any kind, but what they can be detected.

So it seems to me that this committee should not be bound entirely by just a matter of seismology. But that they also should think in a somewhat different field than geophysics about how much communication of information to people occupying these stations can take place through other contacts that people make.

Representative HOLIFIELD. You are speaking now of espionage. This is exactly what the Soviets are afraid of and why they will not allow the stations behind their border.

Dr. UREY. Maybe this is the whole question.

Representative HOLIFIELD. All right; but this is not a technical question.

Dr. UREY. If you can break down that one single thing the problem would be solved.

Representative HOLIFIELD. This is not a technical question.

Dr. UREY. That is right.

Representative HOLIFIELD. This committee is trying to get the technical facts on the line and we recognize that there is a tremendous body of political judgment that has to be utilized. We are not suggesting that this is the only field. We are not in the position on this committee of holding these hearings for the purpose of passing judgment on the nontechnical field.

Dr. UREY. I agree with that.

Representative HOLIFIELD. The purpose as outlined in our opening statement clearly defined that we are concerned with the technical scientific facts. The reason for having these facts is so that they can become part of the judgment to balance against the political judgments and judgments made for other reasons.

Representative HOSMER. In other words, we are not in the field whether we should man these stations with lucky old prospectors.

Dr. UREY. Mr. Holifield, I recognize what you say. However, as you will note, the number of inspection stations was growing so large in the recent testimony before the committee that I think it is time to recognize there are other types of observation.

Representative HOLIFIELD. I think we do recognize that, sir. The only point is that we are not trying to control the testimony as to technical judgments. If the technical judgment says there needs to be 600 stations under certain conditions, we are not contesting that. We are not approving it.

We are allowing that public expression to be put on the record. We are hoping any opposition ideas can be put on.

Dr. Bethé, you have been seeking recognition.

Dr. BETHÉ. I would like to make some comment to Dr. Latter's statement, and first to ask him a question:

Assuming that the earthquakes do not have positive motion, how many unidentified events do you think I would have in my net?

Dr. LATTER. Very small, essentially none.

Dr. BETHÉ. What is very small?

Dr. LATTER. Like zero. Between zero and 1.

Representative HOLIFIELD. You are almost as particular about that "very small" as the Chair was about "a few."

Dr. BETHÉ. I just want to point out how conservative I am.

Dr. LATTER. I would like to suggest that maybe you are not so conservative because most of the capability for the system that you are talking about depends upon quite close-in stations. However, the stations that are close in for many earthquakes will see only one of the alternate rarefaction and compression zones. This is likely to mean even more than 10 percent of the earthquakes will show only compressions. This will leave a large percentage unidentified. I chose 10 percent as a reasonable estimate.

Dr. BETHÉ. This is my second comment. I don't understand this. The stations would surround the explosion, would they not?

Dr. BROWN. Not if they were on the coastline.

Dr. BETHÉ. I have never heard as high a factor as 10 percent for this before.

Representative HOLIFIELD. Dr. Carder, you have your hand up?

Dr. CARDER. I would like to ask a question. About that percent, are those stations distributed around the epicenter or are they all on one side?

Dr. LATTER. There are two effects on which I base my estimate of 10 percent. One is that some earthquakes show purely compressive first motions. The other is that most of the seismic events in the Soviet Union occur on the Kamchatka Peninsula and Karafuto Island. Only 40 percent of the events occur in the Soviet interior. For the 60 percent on Kamchatka and Karafuto you are faced with the problem of having stations on only one side of the epicenter.

Dr. ROMNEY. I believe our experience which is based on larger earthquakes than we are talking about would indicate less than 10 percent which give compressions in all directions. It would also indicate less than 5 percent but I cannot be much more specific than that. On the other hand, I certainly would not agree that any geophysical system will ever approach 100 percent efficiency. I say this for some of the reasons you have. In the first place, you cannot surround all of the epicenters, many of which occur on island chains or coastlines or borders of countries. In addition, at the frequency we are talking about—I believe your estimate was of the order of 20,000 earthquakes per year—this means 2 earthquakes per hour, and some of these are going to occur one on top of the other, resulting in extremely confusing signals. So your system will not be perfect. On the other hand, I tend to agree it will be very, very good.

Representative HOLIFIELD. We have a question for Dr. Beers. I am risking you bringing the old prospector in when I ask it. What do you think of the probability of finding by drilling the core of radioactivity from an underground nuclear explosion in an area initially suspected by seismic readings where the suspected area is about 100 square miles?

Dr. BEERS. This involves the whole question of onsite inspection. This means that you have by your teleseismic——

Representative HOLIFIELD. I am assuming that you have the right to drill. I said of the probability of finding it by drilling. We are not going into whether you have that right or not. I am assuming you have the right.

Dr. BEERS. What tools may I employ? Just the drill?

Representative HOLIFIELD. Any on-site inspection tools. You usually use drills, I understand, if they are down in the ground. There is no eruption of gas or other methods of radioactive detection machines at the site at the time it occurs.

Dr. BEERS. I am sorry you restricted my reference to the old prospector because he had something to say about it.

Representative HOLIFIELD. Go ahead and use it again.

Dr. BEERS. Let us take the figures which I gave you. This is like seeing 10,000 anomalies. You have to investigate every one of them before you can quit or until you find your goal.

Representative HOLIFIELD. . I am talking to you now about one anomaly. I am asking you if a suspicious event occurs and you have located the parameter of it within an area of a hundred square miles, what is the probability of finding out what happened there in that hundred square miles by drilling or any other electronic means.

Dr. BEERS. This probability is affected by each step that you take. It improves as you go toward the final goal. At the outset it is 1 in 10,000. As you narrow it down you come to a smaller and smaller target and you gradually improve the probability. If you would stay with it, it will become a certainty. This is the secret of success.

Representative HOLIFIELD. Will you just give us what the procedure would be? We have had a suspicious event occur within a hundred square miles. Let us make it a square 10 miles by 10 miles. How would you go in and what would you do, assuming it is underground?

Dr. BEERS. Having it narrowed down to 10 square miles?

Representative HOLIFIELD. No, a hundred square miles, 10 miles on a side.

Dr. BEERS. Having it narrowed down to a hundred square miles, I would first employ a helicopter or a low-flying aircraft with geophysical instruments aboard which would detect any possible artifact such as a casing or a piece of wire or any metallic substance which had been employed in the explosion. I would employ aerial photography to a scale of 1 to 20,000, which would be used for precision examination from the air.

If an event had actually occurred within the 100-square-mile area, my prediction is that this would narrow you down to a 10-square-mile area or perhaps two or three 10-square-mile areas. I would then recommend that you proceed to examine this 10-square-mile area on the ground in considerable detail. Dr. Foose has a presentation to make.

Representative HOLIFIELD. Maybe we are getting into the onsite inspection. The answer I really wanted to know was what would be the probability of finding it by drilling.

Dr. BEERS. I think I have already told you, sir. If you will follow the entire procedure of onsite inspection until you have covered every anomaly on the 1 square mile your probability will be a certainty. You will find it.

Senator BENNETT. How long would this process take?

Dr. BEERS. It might take several years and it might cost many tens of millions of dollars.

Representative HOLIFIELD. Are there any comments on any of the testimony this morning?

Dr. ROBERTS. I wonder if Dr. Beers' prospector story would not be very effective in the Soviet Union. If you have a location network which has located a suspicious event, you could say yes, this is it, we will inspect it. It places the whole Russian system at the mercy of any defector.

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Dr. BEERS. You get the point of my story.

Dr. BROWN. I think Dr. Roberts in his prospector comment hit on a very important thing. I think we all believe that if the United States signed a treaty and violated it, that fact would become public knowledge very soon. The New York Times, the Joint Committee, or some other responsible group would see to it that this would become known. I think we are talking about the United States there. I think we are not talking about the Soviet Union. I think we all look forward to the day when the same thing will be true about the Soviet Union. When and if that day comes I think we can all be much more confident not only of the efficacy of an agreement of this kind but of the efficacy of any agreement on arms limitation, surprise attack, or anything else. In fact, when that day comes probably such agreements will not be necessary.

Dr. UREY. Mr. Chairman, may I just say to Dr. Brown that I agree with his statement. But there is a certain element probably of defection in the Soviet Union as well. Experts of a different kind than those sitting at this table must be called in to give an estimate of the effectiveness of our inspectors, our stations, operating in a different way than merely reading curves. I think it is the duty of this committee to look into that other technical problem as well. This I think will have the effect of making the network somewhat larger than it would otherwise be. This may in fact make it hopeful that a network of an acceptable kind can be approached. It might just make it success instead of failure.

Dr. OREAR. I would like to give a few technical facts on this particular subject. A few years ago, as far as scientific exchange was concerned, the Soviet Union was completely closed. But now there is a considerable degree of openness. Mainly all I know about it is the exchange of physicists and our contacts with the Soviet physicists. But there are many different international conferences and exchange of visiting scientists. We see large numbers of leading Soviet physicists. Some of these physicists that we see—in fact the leading Soviet physicists—refused to work on the Russian atomic bomb program and were put under house arrest at that time. As far as the scientific community goes, some of the Soviet physicists are not too different from American physicists. We have these international conferences where we talk to them rather closely. There is this openness now.

Representative HOLIFIELD. Dr. Graves, you had your hand up.

Dr. GRAVES. Yes. I would like to agree that if we could somehow or other get very free access to the Soviet Union this would probably be the best control system and the cheapest control system we could have. However, I spent 6 months last year in Geneva arguing with the Russians or helping to argue with the Russians on the question of this control system, and I would like to remind you once more of what Dr. Latter has said. They have not yet agreed to one single control station on their soil. They have not agreed to one single annual inspection.

Representative HOLIFIELD. As a matter of fact, their insistence on the adoption of the principle of the quota and to have the number of inspections politically determined has not varied from the beginning of their negotiations, is that not true?

Dr. GRAVES. Ambassador Tsarapkin at one meeting said; "I don't care how you take these inspections. You can spread them out over the year or you can take all three of them at once." The number he is talking about is nothing like 500.

I am not arguing that therefore one should not proceed here. All I am arguing is that there is in the Soviet mind a very great reluctance to have people wandering at will over the Soviet Union. One should use a note of caution here in thinking about how many we can have.

Representative HOLIFIELD. Dr. Graves, while you have the floor we know that your time is limited. We would like to get a few comments from you on the high-altitude problems and your opinion as to experiments and requirements which we need in this field. We have been talking about underground tests right along. We feel that this is the most important area right now to be considered. Could you in in a few terse comments tell us something about the difficulties or the problems, you might say, which are involved in tests in high altitude and out of space?

Dr. GRAVES. Yes.

Representative HOLIFIELD. You were the adviser to Technical Working Group I on this point, and I think you are qualified to speak.

Dr. GRAVES. This again is a region where much research is required. There is a lot going on in space which we are just beginning to find out about. At my laboratory we are setting up some instruments to see what would be possible from ground-based stations. It looks to us as if we would be able to detect a 1 kiloton test at distances like 10^5 (100,000) kilometers. It is probable that one could increase this distance by perhaps as much as a factor of 10. Greater distances than this would be extremely difficult from ground-based stations and we certainly don't know how to do it. If one is talking about distances greater than 100,000 kilometers then one clearly is talking about a system of satellites of some sort. From systems of satellites one can observe as much space as one is willing to send satellites to. We can send satellites now to great distances. However, any research in this field will again be in two directions. You will say, by increasing the sensitivity of my instruments, by increasing the number of satellites, by increasing their distances, I improve my detection capability. On the other hand, by putting shielding on my atomic bombs, by making some effort of concealment, I decrease that detection capability. So one has to worry about the balance of which improves fastest, the detection capability or the ability to conceal. The expense and difficulty of carrying out tests increases as you go to great distances.

On the other hand, the expense and the difficulty of detecting at great distances goes up much faster because the volume goes as the cube of the radius and these great distances result in great volumes to look at.

Representative HOLIFIELD. That is a very terse statement and certainly to the point.

Dr. Bethé, would you care to comment on that?

Dr. BETHÉ. It seems reasonable.

Representative HOLIFIELD. Thank you.

I don't want to leave any member of the panel without an opportunity to make a comment if there is one.

Dr. Carder?

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Dr. CARDER. I would like to make a comment on a matter of several minutes ago concerning the detection capabilities of instruments. There is one item that has been left out in this discussion

It is the matter of looking at seismic events from considerable distances from points not in the Soviet Union but outside the Soviet Union where we have access to a considerable area to set up our instruments. I am thinking of what we call the seismograph array, which consists of a large number of detectors in an area of several square miles, preferably flat granite, if we can find a place like that, which are more or less linked together in a mathematical fashion which will produce the best results. An array like this acts as a wave length filter. It will filter out the noise and the surface waves that cross the array at relatively low speed. But the same frequency waves which have much longer wavelengths, which come in from underneath and have much higher apparent speed will be added. In other words, the faster speed wavelengths will be additive where the noise will be filtered out.

Representative HOLIFIELD. We have had some testimony on that point and I think the general testimony has been that a hundred seismograph instruments would be installed in a station in an array.

Dr. CARDER. I would even say much more than that because relatively low cost instruments can be used.

Representative HOLIFIELD. We haven't got to the cost yet. Does that complete your statement?

Dr. CARDER. Thank you.

Representative HOLIFIELD. Was there any other comment?

Dr. OLIVER. I think the group today has the impression that with additional seismological research we might only be able to improve our capability in this field by a factor of two or three or something like that. I would just like to point out that there are some methods which might pay off more than that by a larger factor. For example, the way to determine the depth of the shocks, very shallow shocks, it might be very important. Although we do not know for sure, it is probably true most earthquakes are deeper than the depths at which we would set off explosions. We now have earthquakes that are deeper than a few tens of kilometers. If we can improve this we can improve our methods of identification a great deal. We can develop seismometers which can go into the ocean bottom and be used to surround these peninsular areas and island so we do not have the trouble of having them within a limited azimuth of the shot. When you were looking at the picture of the seismogram you were looking at the very smallest part of it, the first motion.

Representative HOLIFIELD. I thought you were going to tell me I had it upside down.

Dr. OLIVER. Everyone focused their attention on the very smallest wiggle, the first motion. There is just as much information later and

it goes on for many minutes as the amplitudes go on. The only reason we can't use this information is because we don't know how those waves get there. There is every chance with research that we will get this information.

Representative HOLIFIELD. I gather from that that you agree with the rest, that there should be an aggressive research and developent program.

Dr. OLIVER. Yes, sir.

Representative HOLIFIELD. Could you put your prediction as to improvement in terms of years?

Dr. OLIVER. No, sir. Some of these things might be developed in a few months. You might learn methods to detect the depth of shocks within a few months and then again you might not. You cannot predict that.

Representative HOLIFIELD. You cannot predict when a scientific breakthrough will occur in a certain area.

Dr. OLIVER. No, sir.

Representative HOLIFIELD. You cannot order it, either, like an on-the-shelf item.

Dr. OLIVER. No, sir.

Representative HOLIFIELD. Gentlemen, I think we will adjourn now until 2 o'clock. I want to thank each and every one of you for participating in this panel. I know you have done so at great trouble and expense of time and money in order to come here.

This afternoon we will continue the hearings by receiving testimony on the procedures, capabilities, techniques, and limitations of onsite inspection. Our witnesses will be Dr. Foose, Stanford Research Institute, Dr. Roland Beers, petroleum consultant, and we may announce others later.

(Whereupon, at 12:10 p.m. the committee was recessed, to reconvene at 2 p.m. the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order. We will open this afternoon's session with testimony on the procedures, capabilities, techniques, and limitations of onsite inspection. Our first witness will be Dr. Charles Violet, Livermore, Calif., who will testify on the capabilities of onsite inspections to include (1) methods of testing; (2) the nature of the underground zones. Dr. Violet is actually presenting the testimony originally scheduled for Dr. Gerald Johnson of the Livermore Laboratory, who has been forced to leave due to a previous pressing commitment in the Middle West. We are happy to hear you, Dr. Violet, and we realize that you are capable of making such a presentation in your own right.

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STATEMENT OF DR. GERALD JOHNSON,[1] LAWRENCE RADIATION LABORATORY, LIVERMORE CALIF., PRESENTED BY DR. C. E. VIOLET [2]

Dr. VIOLET. Mr. Chairman, members of the committee, it is an honor to have this opportunity to discuss underground nuclear testing and the effects of underground explosions with you.

This background information is necessary to set the stage for consideration of the technical aspects of onsite inspection.

The problem of onsite inspection involves consideration of "unusual human activity," which is a consequence of the magnitude of construction efforts and operations in the field and of the technical features of underground explosions. In my discussion I will cover these two general areas. Dr. Beers and Dr. Foose will then discuss the procedures and techniques of onsite inspection pertinent to underground testing.

That underground nuclear weapons development tests can be successfully conducted with no radiological hazard due to fallout was demonstrated 2½ years ago. Proven techniques are available for obtaining almost all of the desirable data for the use of weapons designers.

The required measurements, the degree of accuracy, and the range of coverage, determine the feasibility and the complexity of the diagnostic system to be employed. As a rule, the overall measurement system is dictated by the requirements of the weapons designer but usually is modified in various ways to provide insurance such as the use of multiple channels, the employment of independent methods for obtaining the same data, and for diagnostic technique development.

The tunnel facilities used in Nevada for the Hardtack Phase II series of September to October 1958 consisted of two complexes. One tunnel system, designated as U12b, had a maximum overburden of 900 feet, and the other, designated as U12e, had a maximum of 1,400 feet. The two tunnels are shown in the first two figures. The first figure (fig. 1A, p. 268) shows what we call the U12b complex, showing the main access tunnel and side drifts used for nuclear weapons test experiments. The second figure (fig. 2A, p. 269) shows the

[1] Born in Spangle, Wash., and attended elementary and high school there. Subsequently he attended Washington State College (B.S.,1937; M.S., 1939 in physics) and the University of California at Berkeley, where he obtained a Ph. D. in physics in 1947. In the period 1941–46 he served in the Navy at the Naval Proving Ground in Dahlgren, Va., where he carried out experimental development work in armor and armor-piercing projectiles.

At the end of the war he returned to the University of California to complete his graduate work and then he became an assistant professor of physics at Washington State, where he taught until 1949.

From 1949 to 1951 he carried on research on crystal growth and neutron physics at the AEC's Brookhaven National Laboratory in Upton, N.Y. In 1951 he went to Washington and worked in the nuclear weapons effects field at the Armed Forces special weapons project. After serving for a short time as a special assistant to the Director of Research of the AEC in 1953, he joined the physics department at UCRL. He was in charge of nuclear test operations for the laboratory for 5 years. During that period he participated in four weapons test programs both in the Pacific and in Nevada. Dr. Johnson was the test director for the Nevada Test Organization for the last two weapons series in Nevada. In this capacity he was responsible for the scientific phase of the operations.

He has participated in the investigations of peaceful applications of nuclear explosives, known as the Plowshare program, since its inception. As associate director of Lawrence Radiation Laboratory he now guides the Plowshare technical program as well as test activities. Demonstration experiments under consideration include the large-scale excavation experiment in Alaska, recovery of oil from Colorado oil shales, recovery of oil from tar sands in Alberta, and an experiment in Carlsbad, N. Mex., to investigate the possibility of utilizing the heat of an atomic explosion for power or process heat and the neutrons to make isotopes.

[2] Physics: Des Moines, Iowa, May 1, 24; m. 51; c. 1 B.S. Chicago, 48; A.B. California, 49, Ph. D. (physics, 53, Teaching asst. physics, California, 49–50, physicist, Radiation Lab, 50–A.F, 43–46, 2nd Lt. Electron) electron scattering; positron-electron scattering; multiple and discrete scattering of electrons; grain density measurements of electrons in nuclear emulsion.

U12e complex which had during that series two experimental side drifts. The next (fig. 1, p. 240) shows the relative locations of the various explosions in a vertical cross section of the mountain.

Representative HOLIFIELD. Where is the tunnel in that illustration?

Dr. VIOLET. The tunnel would be superimposed directly on this profile. This is a vertical section through the U12b tunnel.

Representative HOSMER. Where do the explosions occur?

Dr. VIOLET. The circles indicate the zero points of each explosion. Actually this is a profile through B tunnel. The E tunnel is about 2,000 feet away and those zero sites have been projected on this same profile.

Experience gained from these nuclear explosions has permitted the conclusion that in this formation the radioactivity from nuclear explosions can be contained underground for scaled depths equal to $400W^{1/3}$ feet where W is the energy release in kilotons. Thus, for complete containment, 1 kiloton would require 400 feet of overburden; 20 kilotons, 1,080 feet; and 100 kilotons, 1,850 feet.

The range of complexity of diagnostic systems used underground to meet the widely separate experimental objectives can be illustrated by the setups used for the Blanca and for the Evans events of the operation in Nevada of the fall of 1958. Both of these shots were weapons development tests.

Blanca was a minimal experiment directed toward development of a nuclear weapon. The general layout is shown in figure D (fig. 5A, p. 272). The yield from the experiment was 20 kilotons. The desired measurement was the reaction history using conventional techniques. A single detector system obtained this minimum data in the same recording system. The radiochemical yield was determined through postshot drilling and laboratory analysis.

Evans was a highly instrumented experiment in weapons design. The instrumental layout was as shown in figure E (fig. 8A, p. 275).

We see here a number of lines of sight in blue which were drilled through the rock from drilling stations shown as an extension of the access tunnel. These lines of sight were used to measure the prompt radiation that takes place during the nuclear reaction. Here we have, you see, many lines of sight. For Blanca we had only one line of sight.

For underground testing purposes, the Los Alamos Scientific Laboratory has used 500-foot vertical drilled holes for staging development experiments in Nevada. Similar methods are planned for the seismic improvement explosions and for some of the Project Plowshare experiments.

The general arrangement for operations in a vertical drilled hole would include a diagnostic capsule superposed on the explosive, connected to the surface by cable through which the data would be transmitted. Line-of-sight holes, with appropriate collimation, could be installed for more detailed measurements. Radiochemical samples could be recovered either by permitting bomb debris to be projected through vacuum pipes up to filter recovery systems or by drilling later into the radioactive zone.

This discussion provides the background information on the procedures which would be followed in testing underground.

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Representative HOLIFIELD. How many days did it take you to drill into this to test the radioactivity?

Dr. VIOLET. The first hole in the Blanca event was completed approximately 1 month after the explosion. However, radioactivity was encountered after drilling a distance of about 70 feet after a period of about 3 days of drilling. Activity from the explosion was projected down the Blanca side drift and into adjacent cracks. Since we were drilling parallel to and adjacent to this side drift we encountered radioactivity after a relatively short drilling distance.

Representative HOLIFIELD. Were you using a standard rotary type oilfield drill?

Dr. VIOLET. No, sir. We use diamond drilling techniques specially adapted to take care of the radioactivity problem.

Representative HOLIFIELD. That was not the old cable tool type of drilling; was it? Was it not a rotary bit?

Dr. VIOLET. Yes; with a diamond bit on the end, with an inner tube for core recovery.

Representative HOLIFIELD. That is exactly the same type of drill that is used in oil-well exploration.

Dr. VIOLET. For exploration; yes. You are correct, sir.

Representative HOLIFIELD. What size was the hole that you drilled?

Dr. VIOLET. Approximately 3 inches. It was an NX hole which makes a 3-inch hole in about a 2-inch core.

Representative HOLIFIELD. You went down about 500 feet?

Dr. VIOLET. For the Blanca debris we drilled horizontally for a distance of approximately 700 feet.

Representative HOLIFIELD. And your vertical line was how much?

Dr. VIOLET. We did not drill vertically into these events. We were able to obtain access close enough to these events from the main tunnel. So we set up in the main tunnel to drill horizontally.

Representative HOLIFIELD. Then you must have had your test in a mountain.

Dr. VIOLET. That is correct, sir.

Representative HOLIFIELD. Was the Rainier shot you drilled vertical?

Dr. VIOLET. Yes, sir.

Representative HOLIFIELD. Is it or is it not true that you missed your objective in drilling vertically?

Dr. VIOLET. We didn't miss the objective but we missed the zero point by 16 feet out of 900. We did obtain radiochemical information from this vertical hole. However, most of the valuable radiochemical information for the Rainier event was obtained by horizontal drilling from the tunnel.

Representative HOLIFIELD. This would not be available, of course, in tests that were held on the level. You would have to drill vertically and drilling operations would have to be pretty accurate?

Dr. VIOLET. That is correct. You would at least have to survey your holes to know where you were.

Representative HOSMER. I take it that these figures you gave for the required depth to prevent radiation from being detected from the surface would be approximately the range, from a horizontal standpoint, by which your drill hole could be off and still detect information: is that right—or would it have to be closer in to point zero?

Dr. VIOLET. These are the vertical depths one would have to drill for an experiment designed on this depth criteria. I will get into the pertinent radii of interaction later.

For example, in a 1.7 kiloton explosion, one has to hit a diameter of approximately 120 feet in a depth of, say, 400 feet.

Representative HOSMER. Then the penetration of the radiation is not spherical; it is more up than it is to the side.

Dr. VIOLET. Initially it is spherical but later it is aspherical due to collapse of this initial cavity, I will describe that.

Representative HOLIFIELD. If you had not known in advance the exact location of the test, would you have been able to find it by drilling?

Dr. VIOLET. Given enough time and enough drills, certainly.

Representative HOLIFIELD. Do you mean by that that you might have had to drill a number of holes, each of which would have taken a month, in order to hit the point zero?

Dr. VIOLET. I know the diameter of the target circle I must hit for a 1-kiloton explosion in tuff. For a given area, if I know an explosion is in this area, and I know roughly the yield, then I know what the target circle is. If this is all I know, then I could very easily calculate the probabilities.

For example, take the question that was raised this morning. Armed with a drill, what are the probabilities of locating radioactive debris from, say, a 1-kiloton explosion? It turns out that this is approximately 1 part in 100,000 of hitting with the first hole. To make the probability 1, or equal to unity, then one would have to drill 100,000 holes.

Representative HOLIFIELD. Is it being ridiculous if there are 500 unidentified events that you wish to explore that you might have to drill many thousands of holes in order to locate them?

Dr. VIOLET. Sir, I am not qualified to answer that question. The on-site inspection presentation will probably present to you to what degree this factor can be reduced. Presumably we are armed with more than drilling instrument as a tool. We have other tools to reduce this or to improve our inspection capabilities.

Representative HOSMER. That would lead you to a probable more fruitful location of putting the hole?

Dr. VIOLET. Yes.

Representative HOLIFIELD. But without that information it is 1 in 100,000?

Dr. VIOLET. That is right.

Representative HOLIFIELD. Just by the laws of chance.

Dr. VIOLET. Just by probability.

Representative HOLIFIELD. What are you speaking of when you say you have other information?

Dr. VIOLET. I would defer this question.

Representative HOSMER. That is like construction efforts and technical features of the ground.

Dr. VIOLET. These are aerial surveys, aerial inspection, and other techniques.

Representative HOLIFIELD. You are speaking of intelligence methods rather than of technical methods?

Dr. VIOLET. I am speaking of both kinds. For example, by performing aerial surveys one might isolate anomalies in the earth's surface which can be related to the effects of underground explosion.

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Representative HOLIFIELD. Such as dirt from a mine.

Dr. VIOLET. Yes.

Representative HOLIFIELD. That is what you are thinking of?

Dr. VIOLET. Or visual cracks or temperature anomalies and these kinds of things.

Representative HOSMER. Theoretically anomalies which would be attributable to the explosion itself would be calculated in advance and minimized or eliminated just as you have explained the elimination of radioactivity at the surface. They really would not be available in a smart cheating job.

Dr. VIOLET. That is correct, in my opinion.

Representative HOLIFIELD. You may proceed.

Senator BENNETT. They also might put a few of those around to pull you off and start you drilling in places you were bound to fail?

Dr. VIOLET. Yes, sir.

This discussion provides the background information procedures which would be followed in testing underground for a designed program. However, if a would-be evader of a treaty chose to conduct clandestine tests, procedures could be developed to greatly reduce the scope of the operation. Let us discuss an example of the manner in which such a hypothetical operation might be developed.

Assumptions which must be made concerning a nation intent upon conducting nuclear weapons development tests in a clandestine fashion are as follows:

1. The national policy would strongly support the group carrying out the operation. This would mean that the best technicians would be involved and adequate means would be available, legal and administrative, to minimize changes of detection of intent.

2. The operation would be carried out in such a way that under the provisions of the treaty agreement no inspection would be expected. Yields below threshholds and use of decoupling techniques are two ways this could be accomplished.

3. To meet the conditions of (1) and as insurance in case inspection is inadvertently invited, minimal field operations would be conducted as quietly and quickly as possible and in such a way as to reduce to a minimum the chances of successful site inspection.

Before any nation would embark on a nuclear test program under the conditions concerned, it could first be required to convince itself that new nuclear systems were necessary. The nature of the test program would necessarily depend upon the characteristics of the system to be developed. The design characteristics would depend upon the state of the art in the country and on its strategic objectives. For purposes of illustration, however, it will be assumed that the postulated development program can be conducted with device weights of 2,000 pounds, although greater weights would not be too burdensome.

Once the nation had decided it desired to pursue a development program, it would need to construct a major complex to design and build the devices. The complex would include a large laboratory facility including provisions for high-explosive experiments as well as a supporting industry. For many kinds of nuclear weapons development this might be all that would be needed. However, here we are assuming a nuclear test is decided as necessary to meet the development time scale. The first step of designing and building the 2,000-

pound device package could be done completely within a secure area of the technical complex.

A special firing and monitoring system of minimum weight and high reliability, and one which would require very little attention in the field, would be developed and completely checked out at the laboratory before shipment to the test site.

Since installing cables takes time and requires field support effort, radio firing would be used. Systems for this are already developed and in fact are planned to be used to fire some Plowshare shots. For example, the Alaskan excavation experiment is planned to be fired by radio from an airplane. However, for a clandestine test a specially engineered system would be provided. The control point packaged transmitter and monitor could probably be developed in a weight of not more than 500 pounds, although large weights could be used without creating a problem. At the zero point a small 100-pound unit would mount an appropriate antenna and the auxiliary electronic equipment for firing the system.

To obtain the pertinent diagnostics, fast recording equipment would be required to follow the reaction history. Generally, one would like to have a minimum of 25 recording channels with high-time resolution. Since testing under these conditions would be very important, the reliability of the diagnostic system would have to be high. The system has not, of course, been designed, but individuals at Livermore have given some thought to what could be done by a determined and well-supported group. As in the firing system, if transmission of data to the recording system were required it would be telemetered, either by radio or by light, depending upon the circumstances. Both systems have been used successfully in the field for some types of these measurements.

For the kind of work that has seemed essential to us in the past, a detector or two would be needed at a distance of a few hundred feet from the device. For purposes of concealment, the detector would be placed within the collapse distance of the drill hole or tunnel. The signal from the detector would be telemetered to the recording system outside the damage radius of the explosion.

Thus, in concept the operation would involve the following:

1. The device itself.
2. The collimating system, which determines the lines-of-sight.
3. The detector within collapse distance.
4. The recording equipment outside collapse distance. Data from detector to the recorder by radio.
5. The firing and monitoring system at safe distances. Firing and monitoring by radio.

A schematic possible layout for such an operation is illustrated in figure F (p. 236). The whole system would be thoroughly mocked-up and dry-run at the laboratory before any operations in the field would be undertaken.

The entire operation could be staged in the field in 4 days with 10 or 15 people.

A simple way to execute a clandestine test would be to conduct the operation under conditions such that access to the site could be had by water. The shot point itself would be in a drilled hole, or a washed-out cavity at the bottom of a drilled hole for decoupled shots, either in the water offshore or on the shoreline. Such a drill hole could be provided using standard oil-well drilling equipment and could be located in a

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shallow sea, a river, or a lake. After construction was finished the test team could approach the site by small boat, lower the device, and fire it. The total time in the field for preparation, firing, and recovering the data would be 4 or 5 days. The staging port could be located several hundred miles from the test site.

One of the largest risks of detection that those carrying out the clandestine tests would have to take in the present stage of development of the test technique would be the leakage of radioactivity to the surface. For each tamped explosions at a kiloton or more today there would be no worry about containment of radioactivity. At lower energy releases, while we believe we know how, we have not demonstrated successful complete sealing either of tunnel or drill holes. In the case of decoupled shots, there has been no nuclear experience. However, we believe that in both of these last two cases, the problem of containment could be solved and the techniques could certainly be developed at a few tens of tons using high explosive.

Mr. RAMEY. May I ask a question there?

Representative HOLIFIELD. Mr. Ramey.

Mr. RAMEY. How do you mean on these smaller tests less than a kiloton? You mean the larger explosion would by and of itself seal off?

Dr. VIOLET. That is right.

Mr. RAMEY. And in the smaller it would not and the radioactivity might seep through the debris?

Dr. VIOLET. That is right.

Representative HOLIFIELD. But if you had the tunnels plugged, the plug itself would stop it?

Dr. VIOLET. In these cases the mechanisms in the medium surrounding the explosion essentially shut off and seal up the activity. In other shots that we have fired in Nevada under very pressing circumstances, we were not successful in sealing these explosions merely by putting in plugs. The mechanisms were not strong enough to cause melting and the sealing that occurs for a larger yield. However, we believe if one wanted to be really safe, by filling the tunnel with concrete or imploding the tunnel with high explosives or some technique such as this, we can successfully seal up the radioactivity.

Representative HOLIFIELD. As a matter of act, your last shots were held under a very urgent time schedule in order to predate them before the moratorium; is that not true?

Dr. VIOLET. That is correct, sir.

Representative HOLIFIELD. Your precautions, therefore, were inadequate and more meager than they would have had you been not rushed for time and had you been deliberately trying to conceal them?

Dr. VIOLET. Yes, sir.

Representative PRICE. You described the possibility of leakage of radioactivity to the surface in this clandestine test as one of the large risks of detection. How large a risk is it? Could the release of radioactivity be picked up to the extent that it would be detectable at any great distance?

Dr. VIOLET. In my opinion this is a very small risk.

Dr. VIOLET. I believe as stated in the written text that sealing up the activity under these conditions could be done very successfully so that a leakage would not be detected.

The description of close-in phenomena associated with underground nuclear explosions has been reported in "Underground Nuclear Deto-

nations," by Johnson, Higgins, and Violet, which is attached as an annex (p. 238) and which I propose not to read. Let us summarize the observated results as specified in this publication to define the technical problems of onsite inspection.

Proceeding now to page 8, using the results from the published account of the underground nuclear explosions the target area which any onsite inspection team must locate can be defined by reference to the following figures. Figure G (p. 237) is a cross section through zero point of the Rainier 1,700-ton explosion. There are three major zones of interest:

1. The highly radioactive zone.
2. The collapse zone.
3. The fracture limit.

The highly radioactive zone was formed as a consequence of the collapse of the initial cavity developed by the explosion. The cavity was lined with melted rock which on collapse froze to form a glass containing most of the radioactivity.

Representative DURHAM. When did you run your last test on radioactivity material in that cavity?

Dr. VIOLET. Our last drill hole into the area was probably a year ago. We are continuing to analyze samples from Rainier as well as other zero sites.

The glass collected in a bowl-shaped zone below the center of detonation. The maximum radius of the bowl has been assumed to coincide with the initial cavity.

That is the cavity that was formed by the outward moving shock wave from the explosion. For Rainier the radius was 62 feet.

The collapse zone resulted from the successive vertical collapse of the initial cavity. The rubble zone thus produced had a radius of 62 feet about the zero point at the bottom and spread out upward to a radius of about 75 feet. The height of the zone above the zero point, 5 months after detonation was 386 feet. This zone may have subsequently progressed further but no measurements have been made.

We know that the height of this zone was 386 feet approximately one month and a half after the expolosion on the 1st of November.

The collapse zone was filled with radioactive gases immediately after detonation so that there was radioactivity at a very low level distributed throughout this region.

The fracture limit is the maximum limit at which structural changes occured in the medium near the shot point. There were, in some cases, shifts along geological planes of weakness out to 1,000 feet.

Most of the energy of the explosion is soon deposited as heat surrounding the shot point. The measurements of the resulting temperature distribution for Rainier are shown in figure H (fig. 3, p. 251). One notes that it is almost hemispherical in shape. In principle; an inspection team drilling in the vicinity of an explosion would be able to detect the temperature rise at some point. If we assume that a 3° C. rise above background is detectable, then one would have the following estimated radii of the hemispherical distribution.

Yield:	*Radius (feet)*
100 tons	39
1.7 kilotons	100
20 kilotons	320
100 kilotons	390

From an inspection standpoint, it is necessary to locate radioactivity of the proper age to assure there has been a nuclear detonation corresponding with the time the seismic signal was received. The zone that must be located by an inspection team then is the radioactive zone. All the other quantities, like temperature distribution, discussed above, and the fracture limit, would serve only as indications to guide subsequent drilling to the radioactive zone.

In the following table are estimates of the dimensions to be expected in tuff for the various zones for a range of yields:

Yield	100 tons	1.7 kilotons	20 kilotons	100 kilotons
Radius of radioactive zone (feet)	24	62	140	240
Fracture radius (feet)	50	130	290	500
Vertical collapse (feet)	150	390	870	1,500

Representative DURHAM. What was the actual collapse in this project exposed?

Dr. VIOLET. This collapsed to 380 some feet.

Representative DURHAM. This is for 20 kilotons?

Dr. VIOLET. This is for the 1.7 kilotons. This is for the Rainier event.

Although we have had no experience, we would expect for other kinds of rock that the highly radioactive zone would be smaller and the fracture limits larger than for tuff. The height of collapse would be expected to be quite variable because it probably depends more on the strength of the geological formation than on the details of the explosion.

Finally to avoid the development of visible surface disturbances, the estimated depths required for each explosion are listed below. Note that these depths are two and one-half times greater than would be required to contain radioactivity. If the requirement is to drill vertically to find the radioactivity, then if we use the radius of the fracture zone above the point of detonation, as the defining radius, which is where the radioactive gases would be, one gets the target diameters for various yields listed in the following table. (The chimney diameter above Rainier was 150 feet; all values were scaled from this diameter.)

Yield	100 tons	1.7 kilotons	20 kilotons	100 kilotons
Required depth of burst (feet)	470	1,200	2,700	4,700
Diameter of target (feet)	58	150	340	580

As I mentioned earlier, the diameter for the 1.7 kilotons would be 150 feet, which is an actual measured diameter on the Rainier event.

Representative DURHAM. Does the overlay of earth or rock make any difference?

Dr. VIOLET. It makes some difference because probably the vertical extent depends on some degree on the radius of cracking and this depends on the mechanical elastic properties of the material. For example, in a very hard material such as granite the cracking radius might be larger.

Representative DURHAM. In your statement at the beginning you said 20 kilotons was 1,080 feet overlay and 100 kilotons, 1,850. You use five times the explosive power and your overlay is not but about 800 feet difference.

Dr. VIOLET. You are saying for the 20 kilotons, 1,680.

Representative DURHAM. 100 kilotons, 1,850?

Dr. VIOLET. That is correct.

Here we have for 20 kilotons, 2,700 and 4,700. These we say are about two and a half times this depth criteria which we say we need in order to guarantee no activity appearing on the surface. This latter criteria is to provide that no surface cracking, no visible disturbance appears on the surface. So that must be some factor greater than the vertical extent of collapse.

Representative DURHAM. It is a kind of mathematical matter, then. Taking care of the explosive power is not on a per ratio basis at all with regard to the depth.

Dr. VIOLET. That is correct.

Representative HOLIFIELD. Are there any questions of Dr. Violet?

Representative HOSMER. The development of visible surface disturbances, what do they include? What would a visible surface disturbance be?

Dr. VIOLET. This would be the production of cracks, either visible from an aerial survey or an on-site surface survey. Other things, such as perhaps the vegetation which has had its root system disturbed might die as a result of this. I think the speakers that follow me can describe this more adequately than I can. The attempt here was to point out what we feel would be an adequate distance to prevent visible surface disturbances, such as I mentioned, cracks, effects on vegetation, and so on.

Representative HOSMER. Assuming that you could drill this hole from the inside of a building or something from the middle of town, and below this thing off in the middle of a May Day parade, you would not have much chance at all of any visible manifestations of any kind, would you?

Dr. VIOLET. That is correct.

Representative HOSMER. It probably would not be suspected, either.

Dr. VIOLET. That is right.

Representative HOSMER. Thank you.

Representative DURHAM. Let me ask you this question: After going through with all of these tests and this experience, do you think it could be very easily identified from the surface? I have reference to this operation you have described here.

Dr. VIOLET. The tests we have conducted in Nevada?

Representative DURHAM. I was thinking if you have to go around drilling holes finding these things, if you are going to have a clandestine operation it is quite a job. But if you can walk along the surface and find these things it is a whole lot easier. Is there any identification by walking across the surface?

Dr. VIOLET. Personally I believe this would be very simple to hide on the basis of visual surface observation.

Representative DURHAM. It is just a question of whether you want to go deep enough.

Dr. VIOLET. That is right. That is a question of time and dollars.

Mr. RAMEY. The Rainier shot had some surface indication?

Dr. VIOLET. Yes.

Mr. RAMEY. There were some mountain slides and there was some dust and so on. Of course, that was not a clandestine shot.

Dr. VIOLET. No, sir.

Representative HOLIFIELD. No attempt was made to conceal?

Dr. VIOLET. No, sir.

Representative HOLIFIELD. Thank you very much for presenting Dr. Johnson's paper and also for your contribution.

Dr. VIOLET. Thank you, sir.

(Dr. Johnson's formal statement, and a paper by Drs. Johnson, Higgins, and Violet, on "Underground Nuclear Detonations," follow:)

METHODS OF TESTING

(By Dr. G. W. Johnson)

The problem of on-site inspection involves consideration of "unusual activity," which is a consequence of the magnitude of construction efforts and operations in the field, and of the technical features of underground explosions. In my discussion I will cover these two general areas. Dr. Beers will then discuss the problems of on-site inspection following a postulated clandestine test.

That nuclear weapons development tests can be successfully conducted underground has been amply demonstrated. Proved techniques are available for obtaining most, although not all, of the desirable data for the use of weapons designers. The required measurements, the degree of accuracy, and the range of coverage, determine the feasibility and the complexity of the diagnostic system to be employed. As a rule, the overall measurement system is dictated by the requirements for the weapons designer but usually is modified in various ways to provide insurance such as the use of multiple channels, the employment of independent methods for obtaining the same data, and for diagnostic technique development.

The tunnel facilities used in Nevada for Hardtack Phase II of September to October 1958 consisted of two separate major arrangements. One tunnel system designated as U12b had a maximum overburden of 900 feet, and the other designated as U12e had a maximum cover of 1,400 feet. The two tunnels are shown in figures A and B. (See figs. 1A and 2A, pp. 268–269.) In figure C (fig. 1, p. 240) are shown the relative locations of the various explosions in a vertical section of the mountain. The experience gained from this set of nuclear explosions has permitted the conclusion that in this formation the radioactivity from nuclear explosions can be completely contained underground for scaled depths (D) of $D=400\ W^{1/3}$ feet where W is the energy release in kilotons. Thus, for complete containment, 1 kiloton would require 400 feet of overburden; 20 kilotons, 1,080 feet; and 100 kilotons, 1,850 feet.

The range of complexity of diagnostic systems used underground to meet the widely separate experimental objectives can be illustrated by the setups used for the Blanca and for the Evans events of the operation in Nevada of the fall of 1958. Both of these shots were weapons development tests.

Blanca was a minimal experiment directed toward development of a nuclear weapon. The general layout is shown in figure D (fig. 5A, p. 272). The yield from the experiment was 20 kilotons. The desired measurement was the reaction history as a function of time using conventional techniques. A single detector system obtained this minimum data in the same recording system. The radiochemical yield was determined through postshot drilling and laboratory analysis.

Evans was a highly instrumented experiment in weapons design. The instrumental layout was as shown in figure E (fig. 8A, p. 275).

For underground testing purposes, the Los Alamos Scientific Laboratory has used 500-foot vertical drilled holes for staging development experiments in Nevada. Similar methods are planned for the seismic improvement explosions and for some of the Project Plowshare experiments.

The general arrangement for operations in a vertical drilled hole would include a diagnostic capsule superposed on the explosive, connected to the surface by cable through which the data would be transmitted. Line-of-sight holes, with appropriate collimation, could be installed for more detailed measurements.

Radiochemical samples could be recovered either by permitting bomb debris to be projected up to filter recovery systems or by drilling later into the radioactive zone.

This discussion provides the background information on the procedures which would be followed in testing underground for a well-designed program. However, if a would-be evader of a treaty chose to conduct clandestine tests, procedures could be developed to greatly reduce the scope of the operation. Let us discuss an example of the manner in which such a hypothetical operation might be developed.

Assumptions which must be made concerning a nation intent upon conducting nuclear weapons development tests in a clandestine fashion are as follows:

1. The national policy would strongly support the group carrying out the operation. This would mean that the best technicians would be involved and adequate means would be available, legal and administrative, to minimize chances of detection of intent.

2. The operation would be carried out in such a way that under the provisions of the treaty agreement no inspection would be expected; such as by use of decoupling techniques.

3. To meet the conditions of (1) and as insurance in case inspection is inadvertently invited, minimal field operations would be conducted as quietly and quickly as possible and in such a way as to reduce to a minimum the chances of successful site inspection.

Before any nation would embark on a nuclear test program under the conditions concerned, it would first be required to convince itself that new nuclear systems were necessary. The nature of the test program would necessarily depend upon the characteristics of the system to be developed. The design characteristics would depend upon the state of the art in the country and on its strategic objectives. For purposes of illustration, however, it will be assumed that the postulated development program can be conducted with device weights of 2,000 pounds, although greater weights would not be too burdensome.

Once the nation had decided it desired to pursue a development program, it would need to construct a major complex to design and build the devices. The complex would include a large laboratory facility including provisions for high-explosive experiments as well as a supporting industry. For many kinds of nuclear weapons development this might be all that would be needed. However, here we are assuming a nuclear test is decided as necessary to meet the development time scale. The first step of designing and building the 2,000-pound device package could be done completely within a secure area of the technical complex.

A special firing and monitoring system of minimum weight and high reliability, and one which would require very little attention in the field, would be developed and completely checked out at the laboratory before shipment to the test site.

Since installing cables takes time and requires field support effort, radio firing would be used. Systems for this are already developed and in fact are planned to be used to fire some Plowshare shots. For example, the Alaskan excavation experiment is planned to be fired by radio from an airplane. However, for a clandestine test a specially engineered system would be provided. The control point packaged transmitter and monitor could probably be developed in a weight of not more than 500 pounds. At the zero point a small 100-pound unit would mount an appropriate antenna and the auxiliary electronic equipment for firing the system.

To obtain the pertinent diagnostics, fast recording equipment would be required to follow the reaction history. Generally, one would like to have a minimum of 25 recording channels with high-time resolution. Since testing under these conditions would be very important, the reliability of the diagnostic system would have to be high. The system has not, of course, been designed, but individuals at Livermore have given some thought to what could be done by a determined and well-supported group. As in the firing system, if transmission of data to the recording system were required it would be telemetered, either by radio or by light, depending upon the circumstances. Both systems have been used successfully in the field for some types of these measurements.

For the kind of work that has seemed essential to us in the past, a detector or two would be needed at a distance of a few hundred feet from the device. For purposes of concealment, the detector would be placed within the collapse distance of the drill hole or tunnel. The signal from the detector would be telemetered to the recording system outside the damage radius of the explosion.

Thus, in concept the operation would involve the following: (1) The device itself; (2) the collimating system; (3) the detector within collapse distance; (4) the recording equipment outside collapse distance—data from detector to the recorder by radio; (5) the firing and monitoring system at safe distance—firing and monitoring by radio.

A schematic possible layout for such an operation is illustrated in figure F (p. 236). The whole system would be thoroughly mocked-up and dry-run at the laboratory before any operations in the field would be undertaken.

The packages and occupancy required for transport and installation at the site would be as follows:

	People	Days
Device, 2,000 pounds	2	2
Package for device including monitoring equipment and close-in detectors, 500 pounds	1	
Collimating system, 2,000 pounds	3	3
Detectors and auxiliary equipment, 1,000 pounds	2	
Recording systems, 1,000 to 2,000 pounds	3	2
Firing system, 500 pounds	2	

The simplest way to execute a clandestine test would be to conduct the operation under conditions such that access to the site could be had by water. The shot point itself would be in a drilled hole, or a washed-out cavity at the bottom of a drilled hole for decoupled shots, either in the water offshore or on the shoreline. Such a drill hole could be provided using standard oil well drilling equipment and could be located in a shallow sea, a river, or a lake. After construction was finished the test team would approach the site by small boat, lower the device, and fire it. The total time in the field for preparation, firing and recovering the data would be 4 or 5 days. The staging port could be located several hundred miles from the test site.

One of the large risks of detection that those carrying out the clandestine tests would have to take in the present stage of development of the test technique would be the leakage of radioactivity to the surface. For earth tamped explosions at a kiloton or more today there would be no worry about containment of radioactivity. At lower energy releases, while we believe we know how, we have not demonstrated successful complete sealing either of tunnels or drill holes. In the case of decoupled shots, there has been no nuclear experience. However, we believe that in both of these last two cases, the problem of containment could be solved and the techniques could certainly be developed at a few tens of tons using high explosive.

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Using the results from the published account of the underground nuclear explosions the target area which any on-site inspection team must locate can be defined by reference to the following figures. Figure G (p. 237) is a cross section through zero point of the Rainier 1,700-ton explosion. There are four major zones of interest:

1. The highly radioactive zone.
2. The collapse zone.
3. The fracture limit.

The highly radioactive zone was formed as a consequence of the collapse of the initial cavity developed by the explosion. The cavity was lined with melted rock which on collapse froze to form a glass containing most of the radioactivity. The glass collected in a bowl-shaped zone below the center of detonation. The maximum radius of the bowl has been assumed to coincide with the initial cavity. For Rainier the radius was 62 feet.

The collapse zone resulted from the successive vertical collapse of the initial cavity. The rubble zone thus produced had a radius of 62 feet about the zero point at the bottom and spread out upward to a radius of about 75 feet. The height of the zone above the zero point, 5 months after detonation was 386 feet. This zone may have subsequently progressed further but no measurements have been made.

The collapse zone was filled with radioactive gases immediately after detonation so there was radioactivity at a very low level distributed throughout this region.

The fracture limit is the maximum limit at which structural changes occurred in the medium near the shot point. There were, in some cases, shifts along geological planes of weakness out to 1,000 feet.

Most of the energy of the explosion is soon deposited as heat surrounding the shot point. The measurements of the resulting temperature distribution for Rainier are shown in Figure H (fig. 3, p. 251). One notes that it is almost hemispherical in shape. In principle, an inspection team drilling in the vicinity of an explosion would be able to detect the temperature rise at some point. If we assume that a 3° Centigrade rise above background is detectable, then one would have the following estimated radii of the hemispherical distribution:

Yield	100 tons	1.7 kilotons	20 kilotons	100 kilotons
Radius (feet)	39	100	230	390

From an inspection standpoint, it is necessary to locate radioactivity of the proper age to assure there has been a nuclear detonation corresponding with the time the seismic signal was received. The zone that must be located by an inspection team then is the radioactive zone. All the other quantities like temperature distribution, discussed above, and the fracture limit, would serve only as indications to guide subsequent drilling to the radioactive zone.

In the following table are estimates of the dimensions to be expected in tuff for the various zones for a range of yields:

Yield	100 tons	1.7 kilotons	20 kilotons	100 kilotons
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Fracture radius (feet)	50	130	290	500
Vertical collapse (feet)	150	390	870	1,500

Although we have had no experience, we would expect for other kinds of rock that the highly radioactive zone would be smaller and the fracture limits larger than for tuff. The height of collapse would be expected to be quite variable because it probably depends more on the strength of the geological formation than on the details of the explosion.

Finally to avoid the development of visible surface disturbances, the estimated depths required for each explosion are listed below. Note that these depths are two and one-half times greater than would be required to contain radioactivity. If the requirement is to drill vertically to find the radioactivity, then if we use the radius of the fracture zone above the point of detonation, as the defining radius, which is where the radioactive gases would be, one gets the target diameters for various yields listed in the following table: (The chimney diameter above Rainier was 150 feet—all values were scaled from this diameter).

Yield	100 tons	1.7 kilotons	20 kilotons	100 kilotons
Required depth of burst (feet)	470	1,200	2,700	4,700
Diameter of target (feet)	58	150	340	580

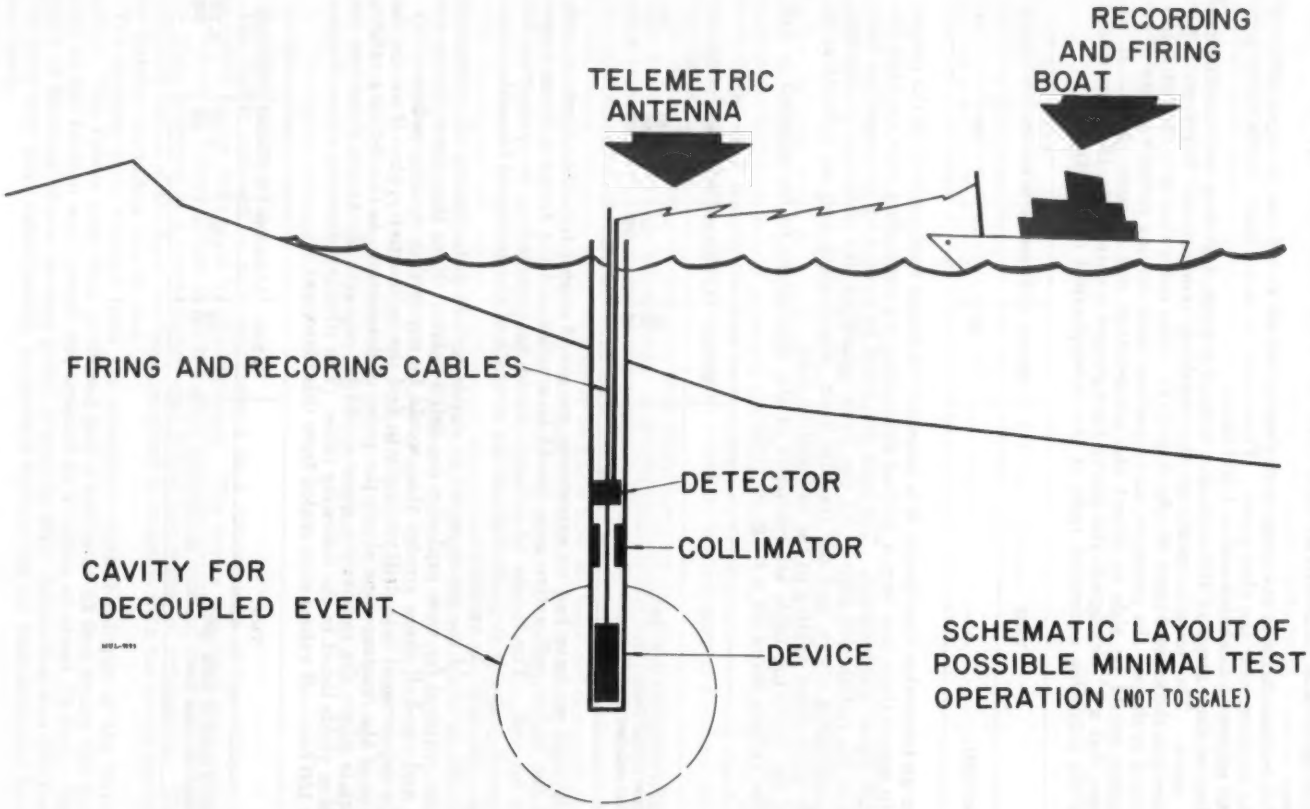

Figure F. Schematic possible layout for minimal field test.

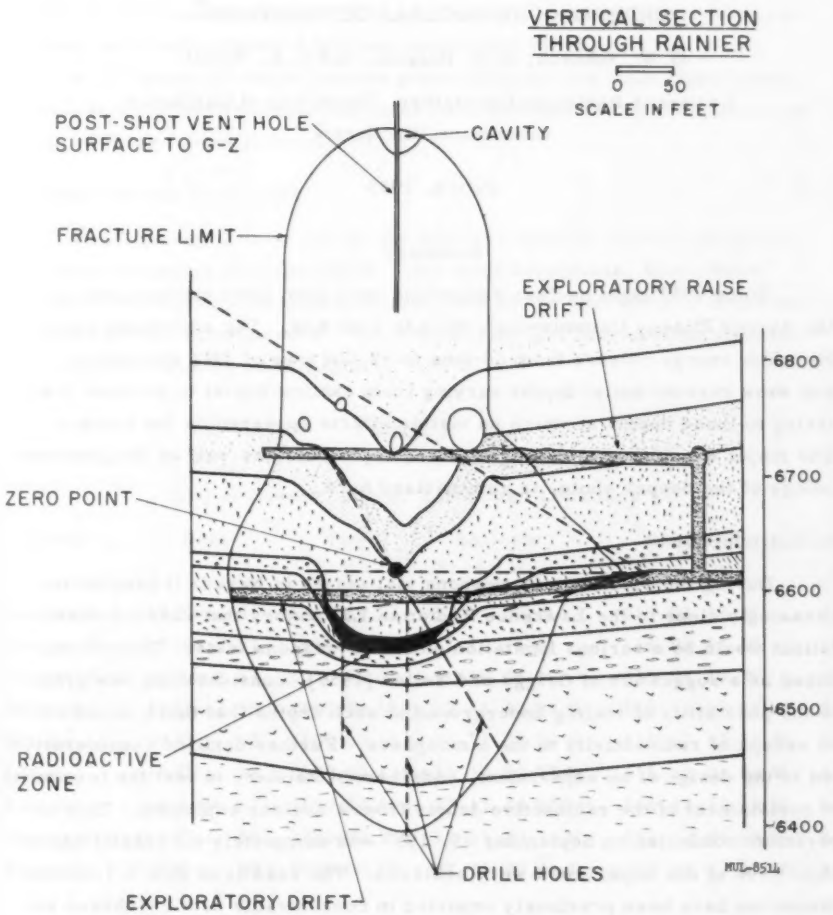

Figure G. Vertical section through the post-shot region of the Rainier event.

Figure F. Schematic possible layout for minimal field test.

UNDERGROUND NUCLEAR DETONATIONS*

G. W. Johnson, G. H. Higgins, and C. E. Violet

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Livermore, California

July 8, 1959

Summary

Since 1952 eight nuclear explosions have been fired underground at the Atomic Energy Commission's Nevada Test Site. The explosions have varied in energy release from 55 tons to 19,000 tons of TNT equivalent and were carried out at depths varying from shallow burial to produce cratering to those depths at which no visible effects appeared on the surface. The major experimental data from these explosions, as well as the phenomenology of the deeper shots, is summarized here.

1. Introduction

During the 1955 nuclear weapons test series in Nevada it became increasingly clear to the Lawrence Radiation Laboratory that concern over fallout would be a serious limitation on future weapons tests. Therefore, based on a suggestion of Griggs and Teller [1956], consideration was given to the possibility of testing underground at such depths that there would be no escape of radioactivity to the atmosphere. Further detailed consideration led to the design of an experiment, code-named Rainier, to test the feasibility of containment of the radioactive debris from a nuclear explosion. This experiment conducted on September 19, 1957 was completely successful and all objectives of the experiment were achieved. The results of this 1.7-kiloton detonation have been previously reported in considerable detail [Johnson and others 1958, Johnson and Violet 1958, Diment and others 1959]. Its continued study has led to a fairly complete understanding of the physical and chemical processes associated with underground nuclear explosions. Subsequent to the Rainier detonation, five additional nuclear devices were fired underground at the Nevada Test Site during October 1958 in connection with

* Work was performed under the auspices of the U.S. Atomic Energy Commission.

weapon development programs. The preliminary results of these later explosions have been published [Johnson and Violet 1958].

In this paper all major results presently available from these shots, as well as three earlier cratering shots, are summarized and the phenomenology of these events is discussed.

2. Experimental Conditions

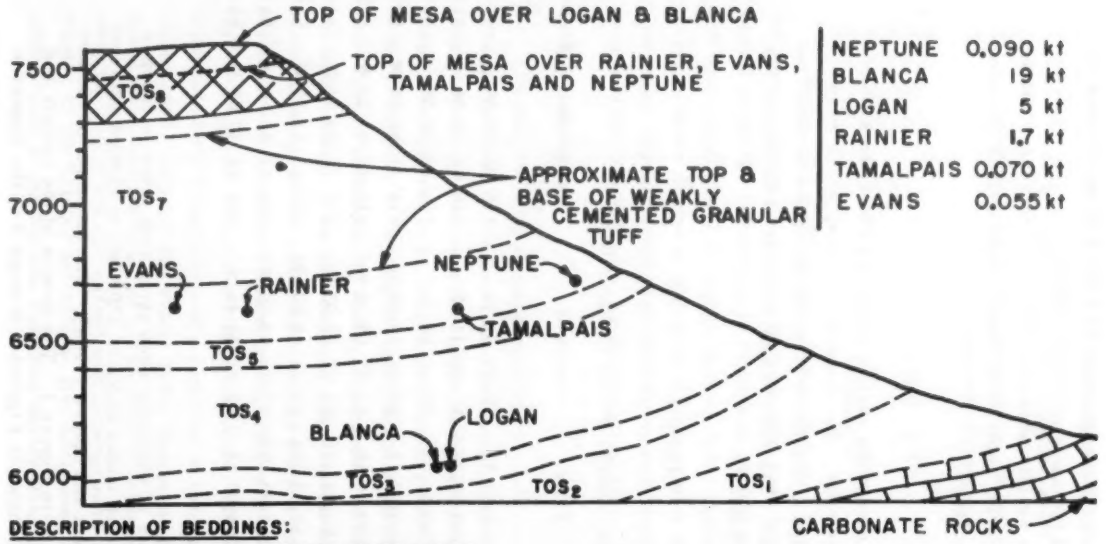
All of the explosions, except the earlier cratering shots, took place in a thick formation of bedded tuffs. The three exceptions, which were detonated in a lightly cemented alluvium, will not be described in detail but are included for completeness. The coordinates and time of detonation for each explosion are listed in Table 1.

TABLE 1

List of Events, Dates, Times and Locations

Event	Date	Time Z	Latitude ° ' "	Longitude ° ' "	Elevation ft
Jangle-S	19 Nov. 51	1659:59.72	37 07 54 N	116 02 19 W	4214
Jangle-U	29 Nov. 51	1959:59.72	37 10 11 N	116 02 33 W	4298
Teapot-Ess	23 Mar. 55	2030:00.01	37 10 06 N	116 02 38 W	4226
Neptune	14 Oct. 58	1800:00.15	37 11 37.88 N	116 11 58.88 W	6716
Blanca	30 Oct. 58	1500:00.15	37 11 09.36 N	116 12 07.28 W	6138
Logan	16 Oct. 58	0600:00.140	37 11 03.03 N	116 12 04.04 W	6141
Rainier	19 Sept. 57	1659:59.454	37 11 44.800 N	116 12 11.35 W	6615
Tamalpais	8 Oct. 58	2200:00.131	37 11 43.10 N	116 12 01.64 W	6616
Evans	29 Oct. 58	0000:00.15	37 11 41.46 N	116 12 17.03 W	6620

The geological structure in which the deep shots took place is characterized by 250 feet of welded tuff under which is 1700 feet of bedded tuffs and a thick bed of dolomite [Diment and others 1958a]. The locations of the several shot points are illustrated in Figure 1. The details of the tunnels and stemming for each event are shown in Figures 1 through 8 of the Appendix. A stratigraphic feature of some importance in interpretation is the loosely consolidated zone in Tos_7, the limits of which are indicated by dashed lines



DESCRIPTION OF BEDDINGS:

TOS_8 WELDED TUFF; RHYOLITE TO QUARTZ LATITE.

TOS_7 FRIABLE TUFF; MOSTLY ALL TUFFS ARE LOOSELY CEMENTED AND "SANDED"; LIGHT GRAY TO GRAYISH BROWN.

TOS_5 BEDDED TUFF; WELL CEMENTED; LIGHT YELLOW GREEN.

TOS_4 BEDDED TUFF; WELL CEMENTED, LIGHT GRAY TO BUFF, SOME PINK.

TOS_3 BEDDED TUFF; WELL CEMENTED, RED AT TOP AND BASE, PINK TO BUFF INTERBEDS.

TOS_2 BEDDED TUFF; MOSTLY LIGHT GRAY TO BUFF.

TOS_1 BEDDED TUFF; PURPLISH TO PINKISH RED.

MUL-7144

GENERAL NOTE:

1. THE DISTANCE RELATIONSHIPS BETWEEN THE ZERO W.P.'s AND THE PROFILE OF THE MESA, ARE CORRECT; HOWEVER, DUE TO THE COMPOSITE NATURE OF THE PROFILE, THE PROJECTED RELATIVE DISTANCES BETWEEN THE W.P.'s ARE NOT CORRECT.

Fig. 1. Profile of mesa showing zero sites.

in Figure 1. Rainier, Evans, Tamalpais, and Neptune were detonated in this lithologic unit about 100 feet below this lower limit. Blanca and Logan were detonated in Tos_3 about 600 feet below this limit.

Physical and chemical properties of the medium, averaged in the vicinity of the Logan, Blanca, and Rainier detonation points are listed in Tables 2 through 7.

TABLE 2

Average Values of Density, Porosity and Water Content
(Errors are standard deviations)

	Logan [Diment and others 1958b]	Blanca [USGS 1958]	Rainier [Diment and others 1959]
Dry bulk density (g/cm^3)	1.8±0.3	1.6±0.2	1.7±0.2
Natural state bulk density (g/cm^3)	2.1±0.2	1.9±0.2	2.0±0.2
Grain density (g/cm^3)	2.6±0.1	2.4±0.1	2.3±0.2
Porosity (percent)	30.6±7.4	33.2±6.0	24.4±7.0
Water content (weight percent)	14.5±3.5	17.5±3.5	15.3±3.5

TABLE 3

Average Chemical Composition
(Percent by weight for air-dry samples)

	Location	
	Logan [Diment and others 1958b]	Rainier [Diment and others 1959]
SiO_2	71.5	66.9
Al_2O_3	13.0	12.3
CaO	0.7	2.3
Fe_2O_3	1.8	2.2
MgO	0.4	1.0
Na_2O	1.3	1.3
K_2O	6.6	2.2
H_2O	4.5	10.6
Balance	0.2	1.2

Fig. 1. Profile of mesa showing zero sites.

TABLE 4

Mineral Composition by Volume Percent

	Logan and Blanca [USGS 1958]	Rainier [Diment and others 1959]
Phenocrysts	19.4	17.4
Quartz	6.9	2.6
Alkali Feldspar	5.6	6.1
Plagioclase	6.0	6.8
Biotite	0.5	1.3
Pyroxene and Amphibole	---	0.2
Magnetite	0.4	0.4
Xenoliths	1.6	6.7
Shards and Lapilli Matrix	70.7	68.4
Heulandite	not differentiated	23
Montmorillomite	"	12
β - Cristobalite	"	11
Amorphous Material	"	22
Vesicles	8.3	7.5

TABLE 5

Thermal Properties

A. Specific Heat* for Rainier Tuff (cal/gram/°C) [Warner and Violet 1959]

Water content, (grams H_2O/ gram dry sample)	Temperature °C: 25	100	200	400	600
0	0.18	0.21	0.24	0.27	0.30
0.15	0.28	0.31	-	-	-
0.20	0.31	0.33	-	-	-
0.30	0.34	0.36	-	-	-

*Calculated from chemical analysis. Does not include heat of vaporization of water.

(continued)

TABLE 5 (continued)

B. Thermal Conductivity ($cal/cm^2/sec/°C/cm$) [Diment and others 1958a]

	Rainier	Logan
Dry	0.0011	0.0014
Wet	0.0016	0.0020

C. Melting Range: 850-1500°C

D. Estimated enthalpy to molten state (≈1500°C): 700 cal/g
Estimated enthalpy to vapor state (≈3000°C): 3000 cal/g

TABLE 6

Average Values of Strength and Elastic Properties

A. Static Tensile Properties of Rainier Tuff [Diment and others 1959]

Air Dry Tensile Strength, psi	165
Air Dry Young's Modulus, psi	0.46×10^6
Air Dry Poisson's Ratio	0.12
Calculated Rigidity Modulus, psi	0.22×10^6

B. Static Compressive Properties of Rainier Tuff [Diment and others 1959]

	Air dry	Natural state	Natural state under hydrostatic pressure 1,000 psi
Compressive Strength, psi	4700	1200	5100
Young's Modulus, psi	1.1×10^6	0.18×10^6	0.37×10^6
Poisson's Ratio	0.11		
Calculated Rigidity Modulus, psi	0.50×10^6		

C. Dynamic Properties of Rainier Tuff [Diment and others 1958a]

f_L	f_T	b	v_L	v_T	$E_{\times10^6}$	$G_{\times10^6}$	σ
5900	4000	0.1	7940	5380	1.07	0.49	0.09

where f is resonant frequency, cps.
L and T are longitudinal and torsional modes, respectively.
b is specific damping capacity determined from width of resonance amplitude curve.

(continued)

TABLE 6 (continued)

v is calculated acoustic velocity, ft/sec.
E is calculated Young's modulus, psi.
G is calculated rigidity modulus, psi.
σ is calculated Poisson's ratio.

D. Bulk Modulus of Rainier Tuff, psi [Warner and Violet 1959]

	Unjacketed	Jacketed
Sample water-saturated	5.7×10^6	0.54×10^6
Average oven-dry sample	4.8×10^6	0.41×10^6

TABLE 7

Miscellaneous Properties of Rainier Tuff

A. Porosity at elevated pressures (average of 2 samples of 28% porosity) [Warner and Violet 1959]

Pressure, psi	0	1000	2000	3000	4000
% Reduction in porosity	0	6%	8%	10%	12%

B. Permeability to Air (millidarcies)[Warner and Violet 1959] to Brine (millidarcie

	Average*	Range**	Average	Range
Rainier	6.0	0.95 - 41	1.4	0.084 - 27
Logan	0.81	0.14 - 2.4	0.036	0.00076 - 0.24

*Averages are computed assuming log-normal distribution.
**Ranges are the total ranges of values observed.

(continued)

TABLE 7 (continued)

C. Seismic Velocity, Vertical Distribution over Rainier [Swift and Sachs 1959]

Interval depth below surface, feet	Interval velocity, ft/sec	Distance above Rainier shot room, feet
230 - 270	7,150	665 - 625
270 - 310	13,700	625 - 585
310 - 395	6,650	585 - 500
395 - 525	7,070	500 - 370
525 - 675	7,180	370 - 220
675 - 775	5,850	220 - 120

(Shot room at depth 895 feet.)

3. General Results

The gross results of the explosions will be described in terms of the visible behavior in the vicinity of the shot point, the escape of radioactivity, the air blast, and the ground shock. The explosions are listed in order of increasing scaled depth in Table 8. The scaled depth is defined as $D/W^{1/3}$, where D is the actual depth in feet and W is the energy release in kilotons (kt) of TNT equivalent. The depth D in the table is the distance to the nearest point on the surface. The column in the table entitled "Measured radioactivity deposited on surface" represents the percentages of the total radioactivity which appeared on the surface, neglecting possible enrichment of particular isotopes.

One kiloton of TNT equivalent is defined as the prompt release of 10^{12} calories of energy (4.2×10^{19} ergs). This energy is determined by multiplying the number of fissions, as measured radiochemically, by the prompt energy release (179 Mev or 2.86×10^{4} ergs per fission). Prompt energy is the sum of the kinetic energy of the fission fragments, and the prompt neutron and prompt gamma ray energies. The delayed energy release due to radioactivity decay amounts to another 22 Mev per fission. About 15 Mev of this energy appears eventually as heat, of which about 7 Mev is released in the first 20 minutes.

TABLE 8

Major Features of Underground Nuclear Explosives

Event	Yield (W) kt	Medium	Depth (D) feet	Scaled Depth $D/W^{1/3}$	Measured radioactivity deposited on surface %	Crater volume yd^3	Crater volume/kt yd^3
Jangle-S	1.2±0.1	Alluvium	-3.5*	-3.3*	>65	1,650	1,400
Jangle-U	1.2±0.1	"	17	16	>80	37,000	31,000
Teapot-Ess	1.2±0.1	"	67	63	90	96,000	80,000
Neptune	0.090±.020	Bedded Tuff	99	220	1-2	33,000	370,000**
Blanca	19.0±1.5	" "	835	310	<0.5	0	0
Logan	$5.0^{+0.2}_{-0.4}$	" "	830	485	0	0	0
Rainier	1.7±0.1	" "	790	670	0	0	0
Tamalpais	0.072±.010	" "	330	780	0+	0	0
Evans	0.055±.030	" "	840	2200	0++	0	0

*3.5 feet above surface.

**This explosion took place in bedded tuff under a sloping surface 1:3 so the crater is probably larger than would be expected on a level surface.

+No breakthrough to surface but radioactive gases in large quantities leaked into the tunnel.

++No breakthrough to surface but stemming failed, releasing gross fission activity into the tunnel.

A. Escape of Radioactivity

In discussing the effectiveness of containment of radioactivity it is necessary to examine not only the escape through the surrounding formation but also the success of the stemming in the tunnels.

For Rainier (1.7 kt) and Logan (5.0 kt) there was no detectable escape of radioactivity to the atmosphere or into the tunnels. From Blanca (19 kt) a chimney developed to the surface and broke out 15 seconds after the detonation. The cloud rose about 1,000 feet and deposited radioactivity on the ground. On integration of the radioactivity in the surveyed area (Figure 9, Appendix) it was found that 0.3 to 0.5% of the gross radioactivity escaped and had been deposited locally. Volatile isotopes were enriched several fold in the vented radioactivity.

The tunnels collapsed at radii of 200 feet (Rainier), 820 feet (Logan), and 850 feet (Blanca) from their respective centers of detonation. There was no detectable leak of radioactivity into the tunnels from any of these explosions. On digging back into the sites it was found that radioactivity had been projected down the tunnels 600 feet for Blanca and 190 feet for Logan.

From this experience it has been demonstrated that the radioactivity released by nuclear explosions can be successfully contained underground. It may be seen from Table 8 that the escape of radioactivity is small at a scaled depth of 300 $W^{1/3}$, and at a scaled depth of 500 $W^{1/3}$ it was not detectable. All radioactivity would probably be contained at a depth of 400 $W^{1/3}$ or greater in tuff.

Neptune, Teapot-Ess, Jangle-U, and Jangle-S, all produced craters and released radioactivity to the atmosphere. In all these cases almost all of the radioactivity that escaped returned to earth within a few miles. With Neptune (Figure 10, Appendix) only 1% of the radioactivity escaped and was deposited near the crater, although the volatile isotopes and those isotopes with gaseous precursors were enriched several fold as noted for Blanca.

B. Local Effects

The ground motion from Rainier was barely felt at 2.5 miles and that from Blanca at 16 miles. There was no structural damage to tunnels or to facilities at or beyond 2500 feet from Blanca. These facilities included a major electronic recording station and a ventilation blower system for the tunnel. These observations give a rough indication of damage radii.

The airblast effects for all shots at scaled depths of 200 $W^{1/3}$ or greater were negligible. These explosions were not audible at a distance of 2.5 miles, although some individuals reported hearing a dull boom from Rainier. While the explosions were not heard by ear, signals were detected by microbarographs in several locations [private communication, Jack W. Reed, Sandia Corp., 1958].

The details of fracturing and bedding plane shifts were mapped and have been described elsewhere [Diment and others 1959]. For Blanca a major section of the top of the mesa shifted down the slope forming a scarp about 70 feet high. The change in profile and area of slumping are shown in Figures 11 and 12 of the Appendix.

C. Earth Motion Measurements

Acceleration and displacement measurements on Rainier by the Stanford Research Institute [Swift and Sachs 1959], Edgerton, Germeshausen and Grier, Inc. [1958] and Sandia Corporation [Perret 1958], indicated that a large earth cap beginning approximately 180 feet below the mesa surface separated from the mesa over the charge and subsequently fell back into place. The only significant vertical displacement occurred at or near surface zero and reached a maximum of 1 foot. The peak acceleration at surface zero was 6 g at 186 milliseconds after zero time.

For Blanca [Swift and others 1959, Morris and Schneiderhan 1959, Perret, to be published] the accelerations measured on the mesa surface show vertical maxima that are consistently larger than the horizontal. The mesa surface directly above the Blanca detonation point was displaced vertically approximately 30 inches in about 400 milliseconds.

Seismic signals from the Rainier event were detected at various stations in the continental U.S. and at distances up to 1000 miles [Diment and others 1959, Carder and others 1958]. Seismic signals were also detected (though barely resolved) at College Station in Fairbanks, Alaska at a distance of 2200 miles.

Seismic effects [Carder and others 1959] resulting from Blanca, Logan, Tamalpais and events of Hardtack Phase-II underground explosions were measured by strong-motion and teleseismic seismographs out to distances of nearly 100 miles. In addition, many temporary seismographs were operated by a number of organizations to distances of nearly 2400 miles, and routine seismographs were operated on a world-wide basis.

Preliminary results indicate that the seismic effects were consistent with empirical formulas developed from Rainier data, except that attenuation of surface waves beyond 200 kilometers may have been higher than the formula for that distance would indicate. Results obtained from the smaller shots require considerably more thought before appraisal. Preliminary seismic data on Logan, for example, indicate that the fraction of energy in the seismic waves is about twice as large as expected from the Rainier formulas.

4. Detailed Local Results

Of the underground explosions, the first one, Rainier, has been studied in most detail and provides most of the data from which a model has been developed. Radioactivity and temperature distributions were determined by drilling and logging a series of holes through the active region. Cores were also recovered and subjected to chemical, radiochemical, and physical property determinations. Similar work, although not in as much detail, has been accomplished for the other explosions. In addition to drilling, two drifts have been driven through the Rainier shot zone. One drift passed about 25 feet below and one about 100 feet above the center of detonation. The layout of drill holes and drifts is shown in Figure 2. Explorations by means of drill holes permitted the determination of the properties of the medium surrounding the center of the explosion.

A. Temperature Distribution

The temperature distribution [Goodale and others 1958, Olsen and others 1959] for Rainier measured five months after detonation is shown in Figure 3.

The energy-content within the various isothermal contours calculated using a mean specific heat of 0.3 cal/g/°C is listed in Table 9.

TABLE 9

Thermal Energy Distribution (Rainier)

Isotherm °C	Average radius feet	Accumulated total energy within isotherm* gram-calories	% of total prompt release
20	100	10^{12}	60
40	80	7×10^{11}	40
60	55	3×10^{11}	17
80	30	6×10^{10}	3

*The temperature distribution was assumed to be axially symmetric.

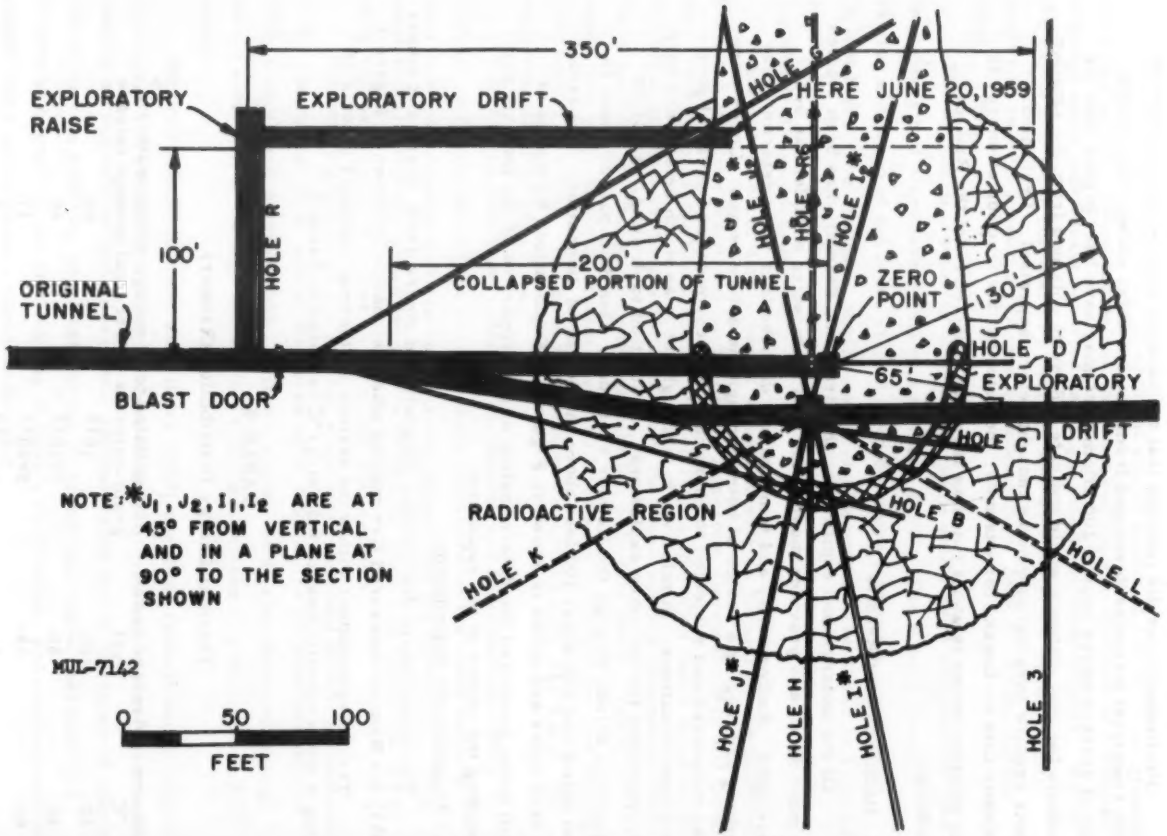


Fig. 2. Drifts and drill holes for post-shot studies of Rainier.

Fig. 2. Drifts and drill holes for post-shot studies of Rainier.

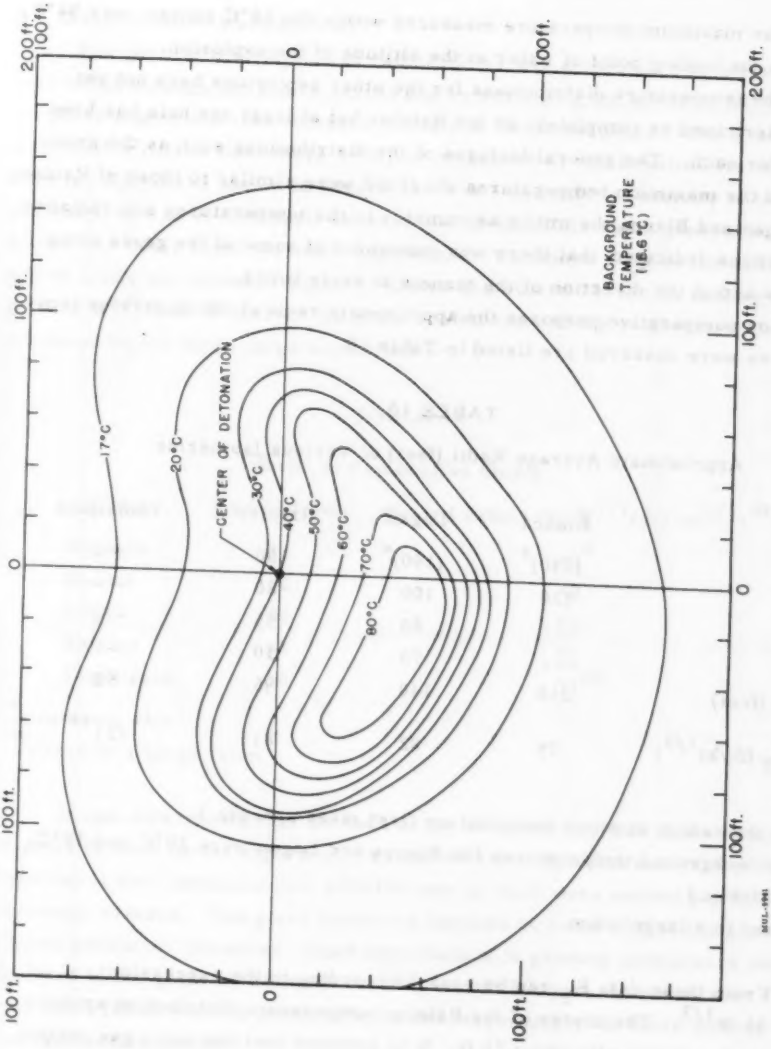


Fig. 3. Temperature distribution of Rainier, five months after detonation.

The maximum temperature measured within the 80°C contour was 94°C, which is the boiling point of water at the altitude of the explosion.

The temperature distributions for the other explosions have not yet been determined as completely as for Rainier but at least one hole has been logged for each. The general features of the distributions such as the geometry and the maximum temperatures observed were similar to those of Rainier. For Logan and Blanca the minor asymmetry in the temperatures and radiation distributions indicated that there was movement of some of the gases along fissures and in the direction of the tunnels at early times.

For comparative purposes the approximate radii at which various temperatures were observed are listed in Table 10.

TABLE 10

Approximate Average Radii (feet) of Various Isotherms

Isotherm °C	Blanca	Logan	Rainier	Tamalpais
20	(240)*	(190)*	100	---
40	120	100	80	---
60	---	80	55	---
80	---	70	30	---
R_T (feet)	210	140	96	50
$\frac{R_T}{W^{1/3}}$ ($ft/kt^{1/3}$)	79	82	81	121**

(R_T is the radius at which temperature first rises sharply.)

*(The background temperatures for Blanca and Logan were 20°C and 18°C, respectively.)

**Fired in a large room.

From these data R_T can be scaled according to the expression: $R_T = 81\ W^{1/3}$. The center of the Rainier temperature distribution appears to be displaced laterally about 20 ft. It is possible that the early gas venting took place in such a direction as to account for this effect. A large open vertical fault did exist 50 feet from that side of the point of detonation, but whether venting actually occurred into this region is not known.

B. Distribution of Radioactivity

The general distribution of radioactivity was determined by logging drill holes through the active region and by surveying the areas of fallout for those explosions that released activity to the atmosphere. The gross distribution [Olsen and others 1959] as measured by thick-walled ion chambers for Rainier is shown in Figure 4. It is noted that the activity is concentrated in a bowl-like shell several feet thick. This same general distribution was noted from the other explosions. The mean radii of the radioactive zones are listed in Table 11 [Olsen and others 1959]. A scaling law $R = 50\ W^{1/3}$ feet is derived from the scaled radii of the radioactive shells produced by the three large explosions.

TABLE 11

Radii of Radioactive Shells

Event	Radius (feet)	Scaled radius ($R/W^{1/3}$ feet/kt$^{1/3}$)
Neptune	21	47*
Blanca	130	48
Logan	85	50
Rainier	62	52
Tamalpais	30	73**

*Cratering shot

**Fired in a large room

It was determined that the bulk of the radioactivity was concentrated in glass formed by the explosion. From the radiochemical analysis of this material it was concluded that 500±150 tons of rock were melted per kiloton of energy release. The glass contained between 60% and 85% of the gross fission products. However, those materials with gaseous precursors were depleted in the glass by as much as a factor of 100 from the nonvolatile species. These volatile isotopes were correspondingly enriched outside the radioactive shells.

Since the fission products are comprised of many elements their volatilities range from very large (krypton, xenon) to very small (zirconium, cerium). The relative abundance of those species which are gaseous and refractory changes as radioactive decay processes proceed after the detonation.

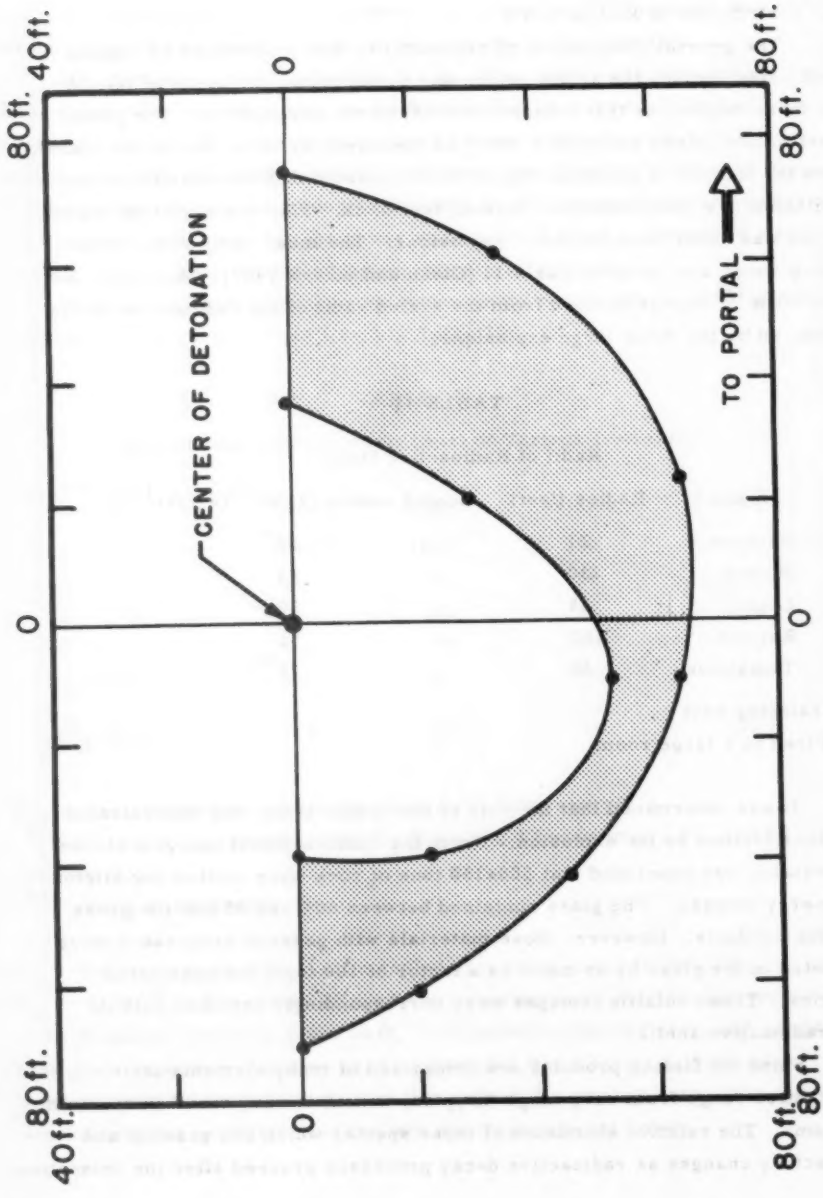


Fig. 4. Distribution of radioactivity — Rainier Event.

Table 12 is a list of the parent-daughter relationships leading to some of the isotopes which were later determined radiochemically.

TABLE 12

Decay Chains Leading to Isotopes Which Have Been Measured in Debris from Underground Explosions

Mass	Element, half-life and measured isotope (underlined)
85	As $\xrightarrow[\text{sec}]{0.43}$ Se $\xrightarrow[\text{sec}]{40}$ Br $\xrightarrow[\text{min}]{30}$ <u>Kr</u>
89	Kr $\xrightarrow[\text{min}]{3.2}$ Rb $\xrightarrow[\text{min}]{1.5}$ <u>Sr</u>
90	Kr $\xrightarrow[\text{sec}]{33}$ Rb $\xrightarrow[\text{min}]{2.7}$ <u>Sr</u>
91	Kr $\xrightarrow[\text{sec}]{9.8}$ Rb $\xrightarrow[\text{min}]{2}$ Sr $\xrightarrow[\text{hour}]{9.7}$ <u>Y</u>
95	Rb $\xrightarrow{\text{sh}}$ Sr $\xrightarrow{\text{sh}}$ Y $\xrightarrow[\text{min}]{10}$ <u>Zr</u>
99	Zr $\xrightarrow[\text{sec}]{30}$ Nb $\xrightarrow[\text{min}]{2.5}$ <u>Mo</u>
137	I $\xrightarrow[\text{sec}]{22}$ Xe $\xrightarrow[\text{min}]{3.8}$ <u>Cs</u>
140	Xe $\xrightarrow[\text{sec}]{16}$ Cs $\xrightarrow[\text{sec}]{66}$ <u>Ba</u>
141	Xe $\xrightarrow[\text{sec}]{1.7}$ Cs $\xrightarrow{\text{sh}^a}$ Ba $\xrightarrow[\text{min}]{18}$ La $\xrightarrow[\text{hour}]{3.7}$ <u>Ce</u>
144	Cs $\xrightarrow{\text{sh}^a}$ Ba $\xrightarrow{\text{sh}^a}$ La $\xrightarrow{\text{sh}}$ <u>Ce</u>

[a]sh – short (compared to a few seconds.)

The effect of the gaseous ancestors in leading to the depletion of certain nuclides from the glass and their enrichment in the vented material is demonstrated in Table 13. These data are the summarized radiochemical results from all of the explosions in tuff.

Fig. 4. Distribution of radioactivity – Rainier Event.

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TABLE 13

Summary of Radiochemical Data from Several Underground Explosions

Isotopes*	Percentage of total in fused material	Degree of enrichment in rubble chimney	Degree of enrichment in vented material***
Kr^{85}	< 1%	~0	All in gas
Sr^{89}, As	3 - 10%	>2	~10
Sr^{90}, Cs^{137}	20 - 40%	>2	~5
Y^{91}, Ba^{140}, Ce^{141}, Cs	30 - 60%	>2	> 2
U, Mo^{99}	50 - 100%	< 2	< 2
Pu, A.E.**, Pa, Hf Ta, Ce^{144}, Nd^{147}	~100%	1.0	1.0

* Examples which have been measured.
** A.E. represents actinide earth.
*** Observed from Neptune and Blanca.

It should be noted that since Sr^{89} and Sr^{90} are strongly depleted in the glass debris, the gases must have escaped from the glass in times comparable to the half-lives of their krypton ancestors (3.2 minutes and 33 seconds). At the temperatures in the cavity, around 1000° or 1500°C, several other fission products are volatile and also appear to be depleted from the glass and enriched in the vented material. Arsenic, cesium, and uranium appear to behave in this way.

5. Phenomenology

The following discussion largely pertains to Rainier since most of the measurements have been completed at that site. Based on the fact that the radioactivity and temperature distributions were concentrated in shells and that within these shells the region was generally permeable to drill water, while outside it was much less permeable, led to the conclusion that the explosions produced cavities of radius $R = 50\ W^{1/3}$ feet, which stood for a short time, then collapsed.

In the case of Rainier, the description of the state of the cavity at this time and its collapse has been described by Kennedy and Higgins [1958]. The cavity when first formed was lined with about 4 inches of melted rock and filled with steam at a pressure of 40 atmospheres, which is approximately the lithostatic pressure. The cavity stood long enough, between 30 seconds and 2 minutes, for much of the fluid rock to flow down the sides and to drip from the roof. At this time the cavity began to collapse and cooled rapidly due to expansion of the steam. The sudden cooling quenched some of the droplets of rock in free fall, as well as some of the 'icicles' as they hung suspended from the cavity roof. The cavity was filled with broken rock from the collapse, and the caving progressed vertically to a distance of 386 feet above the point of detonation.

Inspection of the interior of the collapsed zone revealed the distribution of size of blocks formed. The blocks in the lower drift varied from a few inches in diameter, just inside the radioactive zone, up to several feet in diameter near the center. The regions between the blocks were filled with pulverized material which had resolidified by the time the re-entry was accomplished one year after the explosion. The recementing is not surprising for material with high clay content.

The drift, 100 feet above the zero point, first broke into a block-caved region 75 feet out from the vertical line passing through the center of detonation. At this point the blocks were a few inches to a few feet in diameter with open fractures, then graduated down to a fine powder at a distance of 65 feet. At a distance of 40 feet a large cavity was encountered. In summary the collapse of the initial cavity produced a broken permeable zone that appears to be cylindrical with a radius of 65-75 feet for the first 100 feet of height, and with a total height of about 386 feet. The radius of the collapsed zone above 100 feet has not yet been measured.

Soon after detonation the high temperature rocks rapidly cooled to the boiling point of water (94°C, at this altitude - 6600 feet), because of the water content of this tuff and the large permeable zone resulting from the collapse of the cavity. The diffusion of heat then took place at a lower rate leading to the observed distribution measured five months after detonation (Figure 3).

The Rainier device was detonated in a room 6'×6'×7', which contained about one ton of material. The initial temperature and pressure in the room can be calculated as follows:

$$\text{Energy density: } E = E_p + E_r = \frac{3}{2} n\,kt + \frac{4\sigma T^4}{c}$$

where E_p = energy density in the particles
E_r = energy density in radiation
$n = A_o\rho/M$ = number of particles per cm^3
$k = 1.37\times10^{-16}$ erg/°K (Boltzmann's constant)
T = Temperature °K
$\sigma = 5.74\times10^{-5}$ erg/cm^2/°K^4 (Stefan-Boltzmann constant)
$c = 3\times10^{10}$ cm/sec (velocity of light)
$A_o = 6.04\times10^{23}$ (Avogadro's number)
M = molecular weight
ρ = density in g/cm^3

This can be put in the form

$$E = 3/2 \frac{A_o\rho}{M} k T + \frac{4\sigma T^4}{c}$$

$$= 1.25\times10^{8} \frac{\rho}{M} T + 7.65\times10^{-15} T^4 \quad \text{erg/cm}^3$$

The pressure equals 2/3 particle energy density plus 1/3 radiation energy density thus

$$P = 0.83\times10^{8} \frac{\rho}{M} T + 2.55\times10^{-15} T^4 \quad \text{dynes/cm}^2$$

The prompt energy release of the Rainier explosion was 7.2×10^{19} ergs. Since the mass of material in the room was about 10^6 g and the volume was 7×10^6 cm^3, the mean density was 0.14 g/cm^3. At extremely high temperatures essentially all electrons are stripped from nuclei and since the atomic weight is approximately twice the atomic number, the effective molecular weight is given by

$$M_{eff} = \frac{\Sigma N_i \times 2 Z_i}{\Sigma N_i (Z_i + 1)} \simeq 2$$

where N_i is the number of atoms of atomic number Z_i and the summation is taken over all atoms in the zero room. Applying these numbers as indicated leads to the fact that the temperature a few microseconds after detonation was about 1,000,000°K and the pressure 7,000,000 atmospheres (bars). The radiation pressure at this temperature is 2500 bars.

The calculation of the behavior of the medium from this point onward was carried out by J. Nuckolls [1959], who extended the earlier calculations of Pelsor [private communication]. Nuckolls' treatment involved the development of a code for a computer calculation of the behavior of the medium

from a few microseconds to about 100 milliseconds. This time was sufficiently long to permit calculation of all the dynamic behavior of the system. Only the general results of this calculation will be given here.

It was assumed that tuff has negligible large-scale tensile strength and behaves as a linear elastic solid as long as the tension does not exceed the stress due to overburden pressure. The elastic constants used in the calculation were the measured values for bulk modulus, shear modulus, and sound speed. After the tension in the spherical shell exceeds the lithostatic stress the components of the stress tensor are set equal to a pressure (shear modulus equal to zero) which is related to the volume by a bulk modulus type equation of state.

From the calculations, the shock time-of-arrival was determined. This information together with the measured results of Porzel [Cohn and others, to be published] are shown in Figure 5. In Figure 6 the peak shock pressure as a function of radius is given. It decreases as $r^{-2.35}$ out to about ten meters. The tuff was vaporized to a radius of 2.3 meters in 0.2 milliseconds (peak pressure 1.0 megabar) and melted by shock to 3.3 meters (0.4 mb). Enough energy was deposited by the shock in the first 4.6 meters to melt all the tuff within this radius (660 tons). Radiochemical analysis on Rainier gave about 800 tons. This total amount melted is the sum of the shock induced melting (660 calculated) and the melting induced by fission-product decay energy and should be about 1.2 times larger than the shock induced melting alone. The factor 1.2 results from the ratio of the percentage of prompt energy in gas + liquid + fission product divided by the prompt energy in gas + liquid.

The shock progressed outward and crushed the medium to a radius of 130 feet where the pressure was 1.4 kilobars, or twice the static compressive strength. The elastic radius was calculated to be 285 feet horizontally and 305 feet vertically.

It was calculated that a spherical cavity was formed which expanded outward as the shock advanced. The melted tuff formed the inner surface of this cavity. The calculated rate of growth of the cavity is given in Figure 7. It is noted that it reached its maximum radius of 62 feet in about 80 milliseconds. The radius that was determined experimentally is in agreement with this value.

Nuckolls also calculated the energy and temperature distribution at 90 milliseconds after detonation. At this time, less than 0.5 percent of the energy is kinetic energy and therefore the system is essentially static (Table 14).

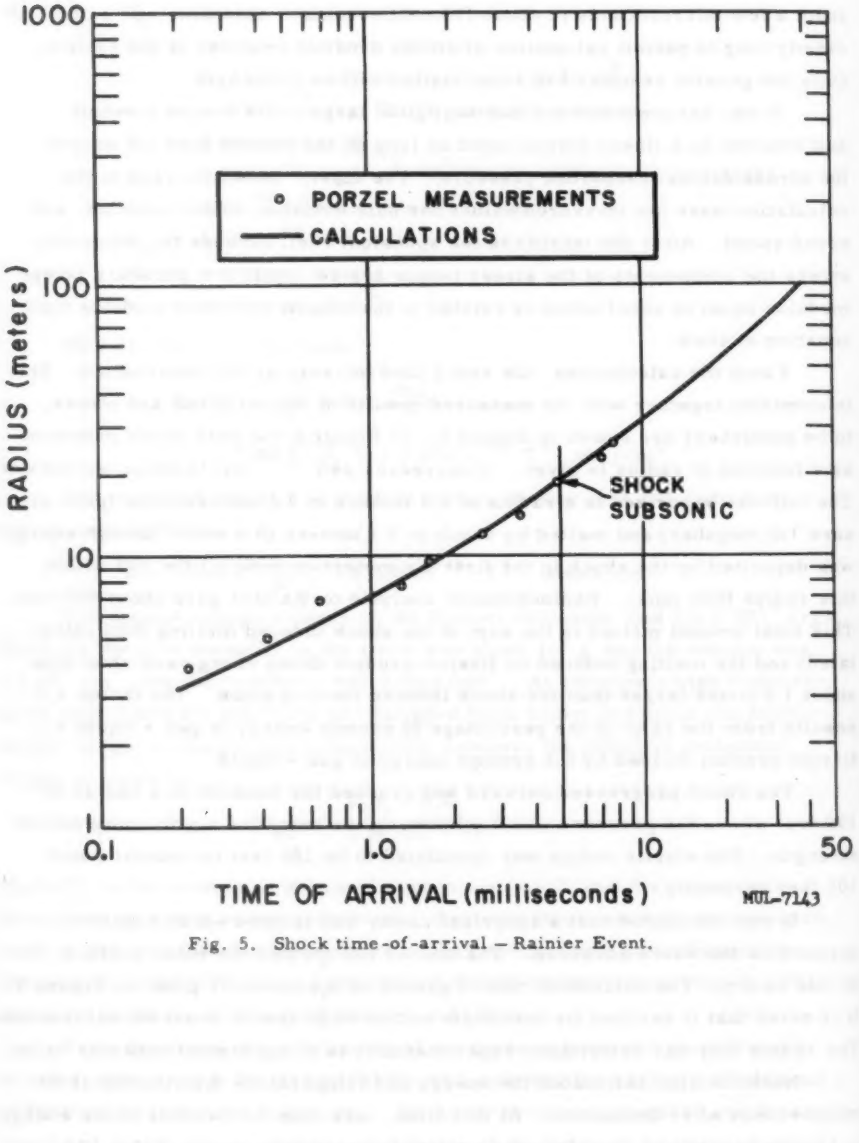

Fig. 5. Shock time-of-arrival – Rainier Event.

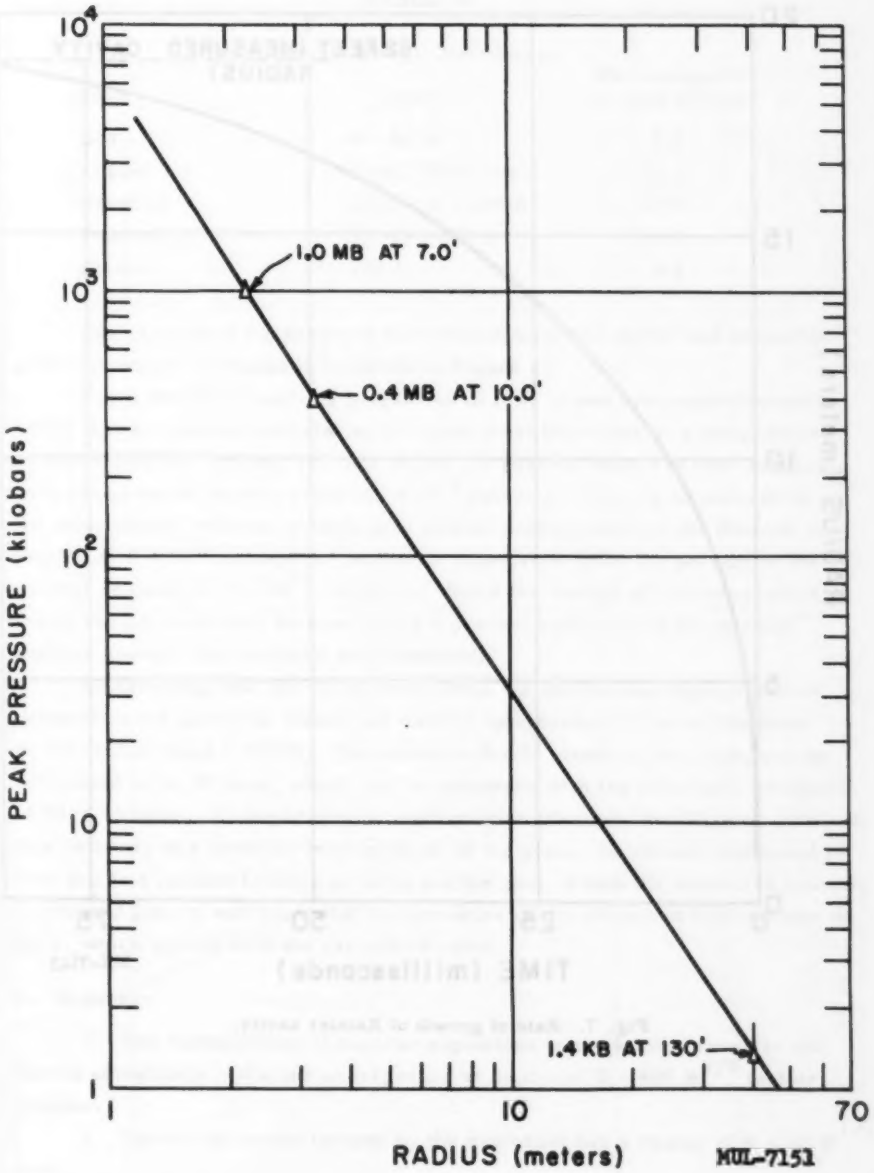

Fig. 6. Peak shock pressure – Rainier Event.

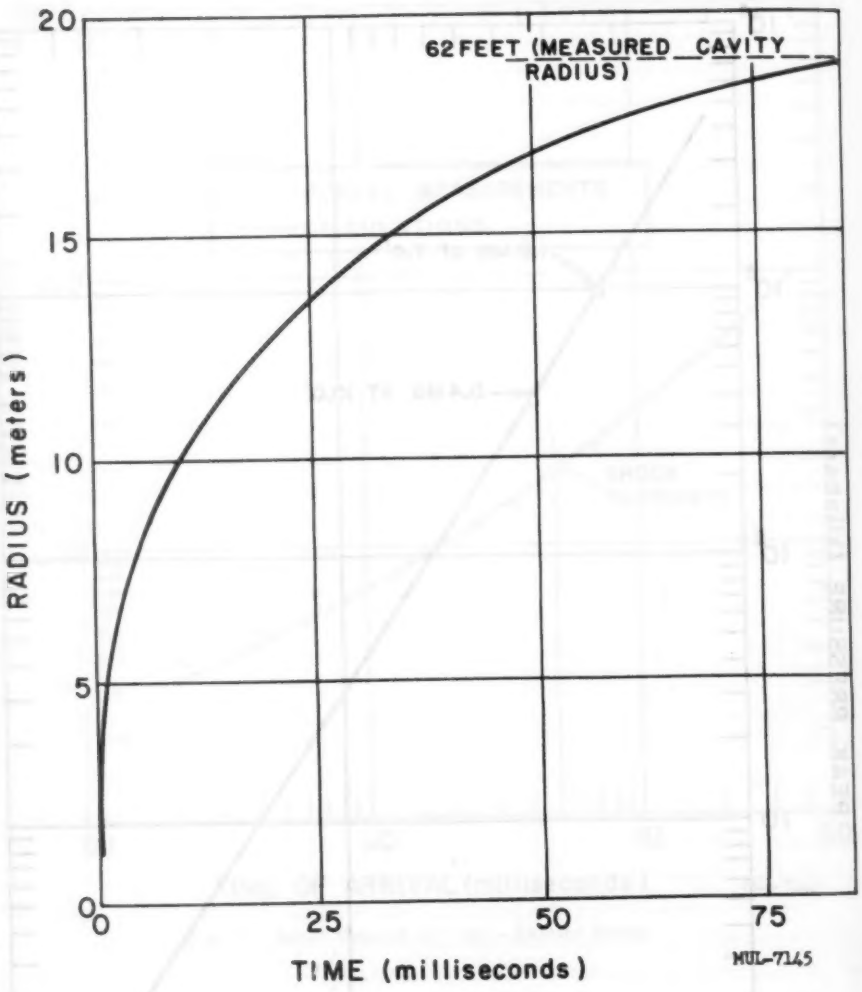

Fig. 7. Rate of growth of Rainier cavity.

TABLE 14

Rainier Energy Distribution

State	Radii	Percentage of prompt energy
Gas	0 - 62 ft	8.2
Liquid	62 ft - 62-1/4 ft	19.1
Crushed	62-1/4 ft - 130 ft	47.0
Fractured	130 ft - 280 ft	21.2
Elastic	280 ft	4.5

The calculated temperature distribution after the cavity had ceased to grow but before it collapsed is shown in Figure 8.

From the known melting properties of tuff, it was concluded [Nuckolls 1959] that the melted material at this time must have been at a temperature of 1200-1500°C. To heat 800 tons of tuff (15 percent water) to this temperature range would require about 5.7×10^{11} calories. This is 32 percent of the total energy release (prompt plus fission product heat) of the Rainier explosion (1.8×10^{12} calories). Nuckolls' calculation gives 27 percent of the prompt release (1.70×10^{12} calories). Since the energy of fission-product decay should contribute between 3 and 4 percent additional to the prompt melting energy, the numbers are consistent.

By assuming that all of the water from the molten and vaporized rock remains in the cavity as steam and that the temperature is about the same as the molten rock (1500°C), the pressure due to steam in the cavity can be calculated to be 40 bars, which can be compared with the lithostatic pressure of 50 to 55 bars. An experimental confirmation [Kennedy and Higgins 1958] of this estimate was made by heating some of the glass, which had condensed in free fall in a vacuum furnace to drive out the gas. From the measured quantity of trapped gas, it was found that the pressure in the contained bubbles was 40 bars, which agrees with the calculated value.

6. Summary

1. The radioactivity of nuclear explosions in the kiloton range in tuff can be completely contained underground at depths of $D = 400\ W^{1/3}$ feet or greater.

2. The initial cavity formed by the explosion has a radius of $R = 50\ W^{1/3}$ feet.

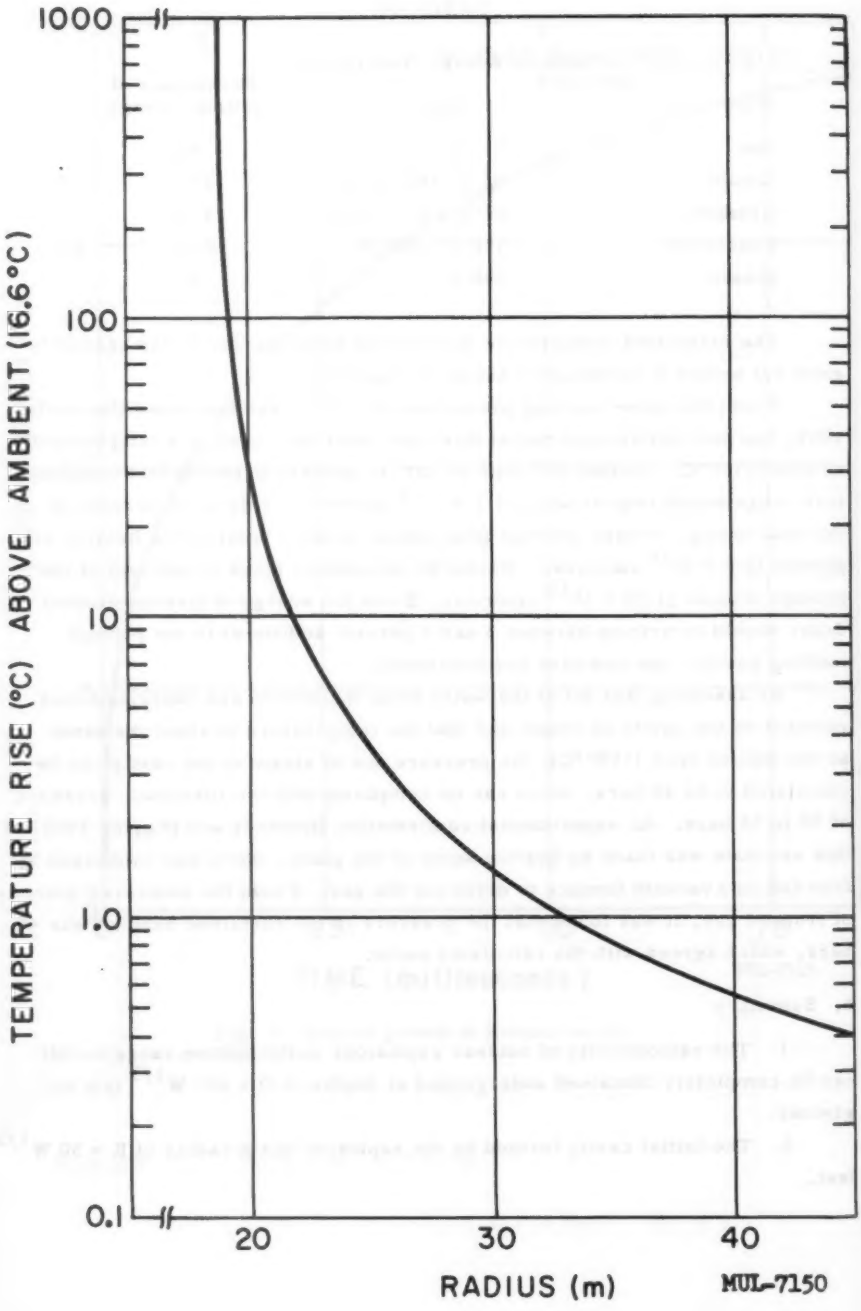

Fig. 8. Initial Rainier temperature distribution.

3. Initially melted rock, which is converted to glass on cooling, amounts to 500 ± 150 tons per kiloton of energy release.

4. The collapse of the cavity produces a zone of about 70,000 yds^3/kt (120,000 tons/kt) of broken permeable material.

5. The major portion (65-80 percent) of the gross fission product activity is in dilute (one part per 10,000,000) solution in glass. The remainder (20-35 percent) is distributed throughout the collapsed zone of the chimney and is deposited on the surface of the broken material.

6. About 30 percent of the total energy release of the explosion is initially deposited in steam and hot rock at a temperature in excess of 1200°C. This temperature rapidly degrades to the boiling point of water. One year after the Rainier explosion this energy resided within a volume whose radius was less than 80 feet.

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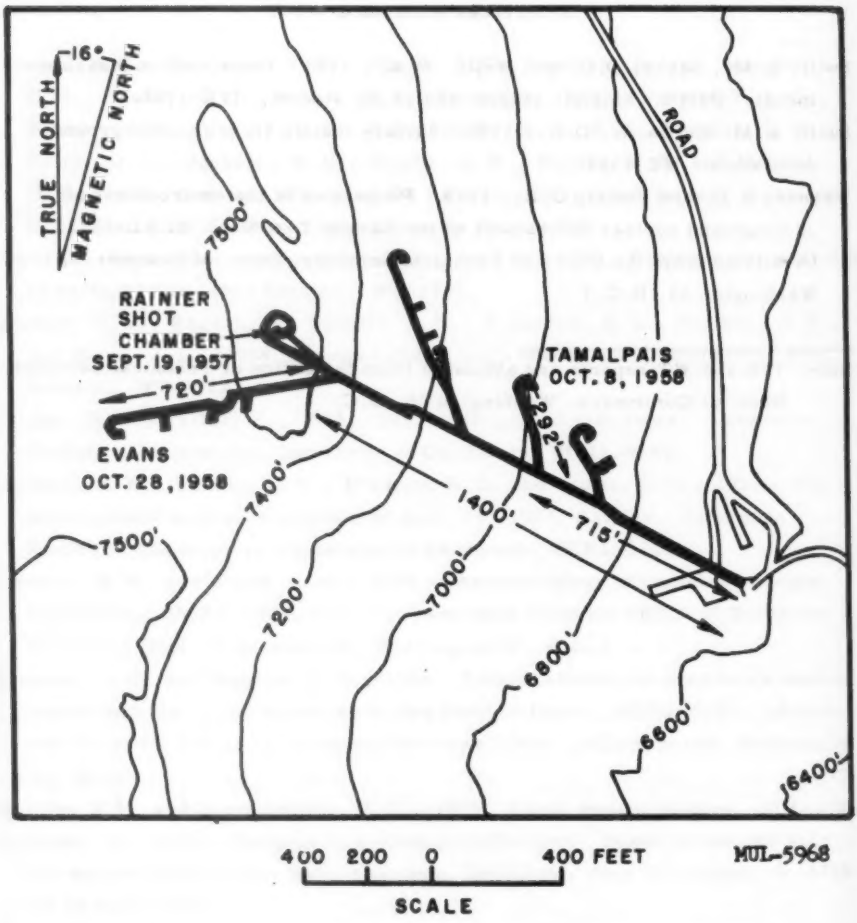

Figure 1A. Plan view of Rainier, Evans and Tamalpais sites.

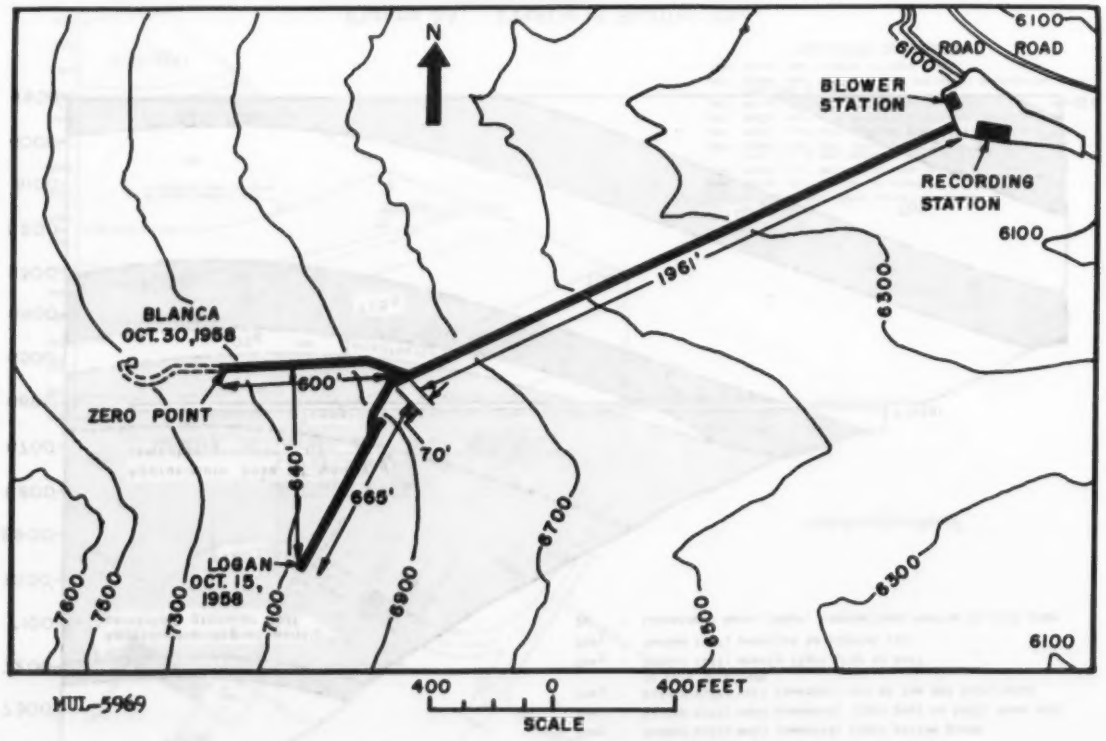

Figure 2A. Plan view of Blanca and Logan sites.

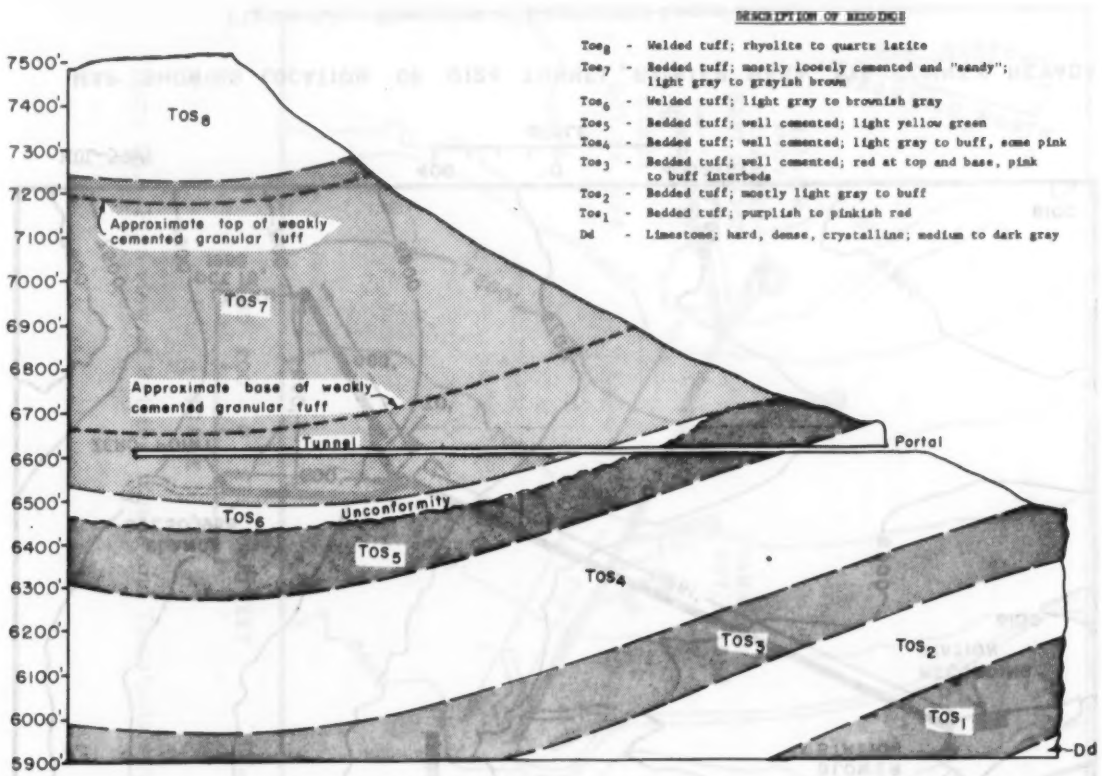

Figure 3A. Profile of Rainier site.

Figure 3A. Profile of Rainier site.

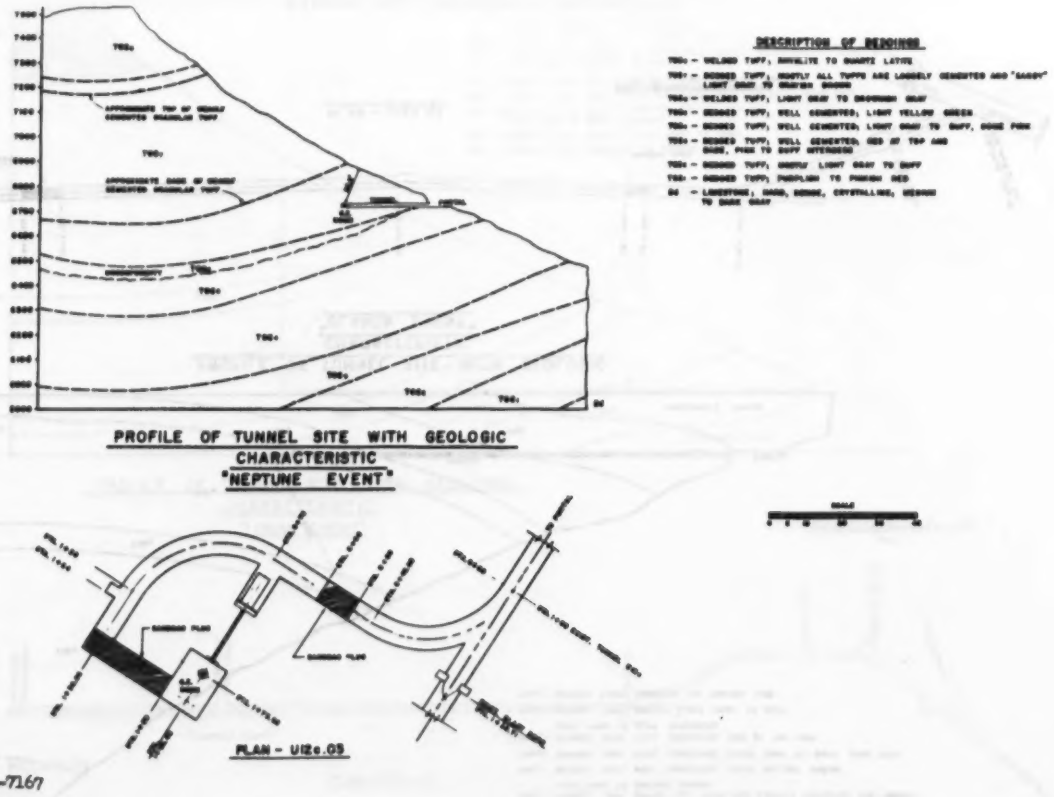

Figure 4A. Plan of Neptune tunnel.

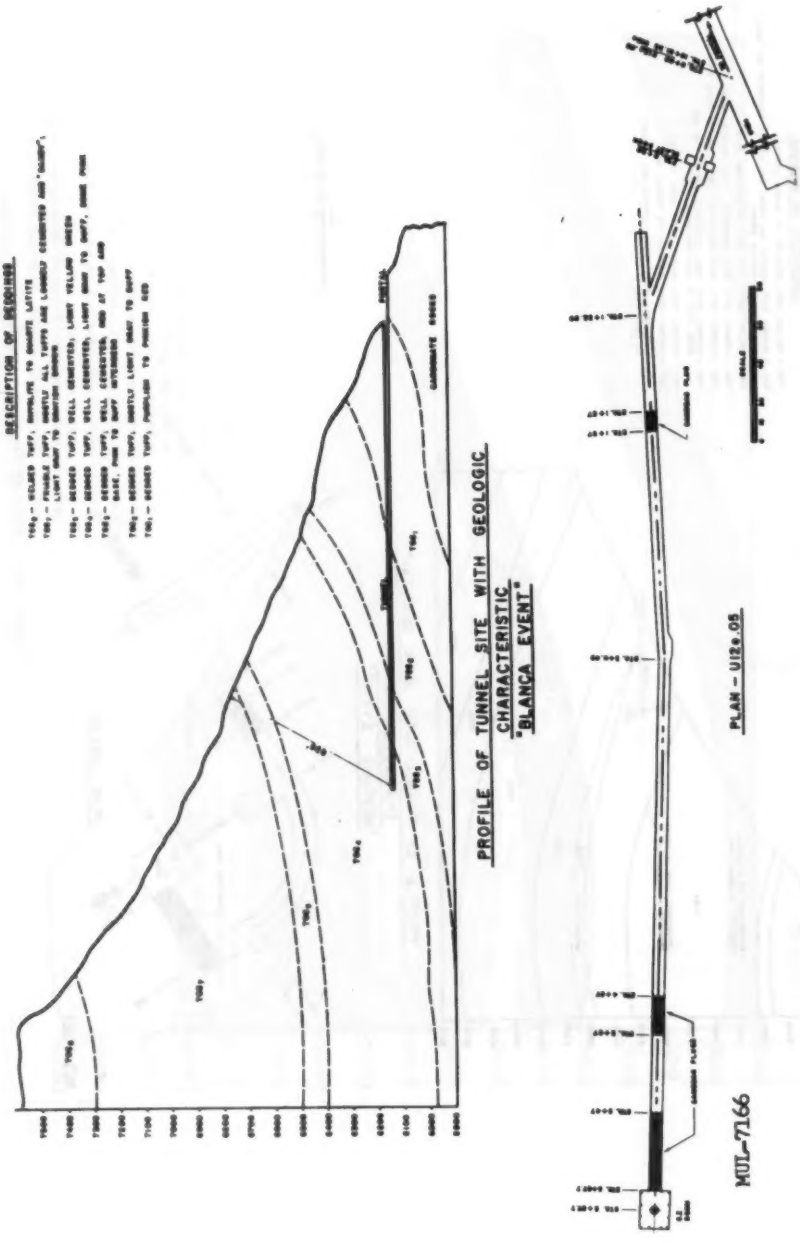

Figure 5A. Profile of Blanca site.

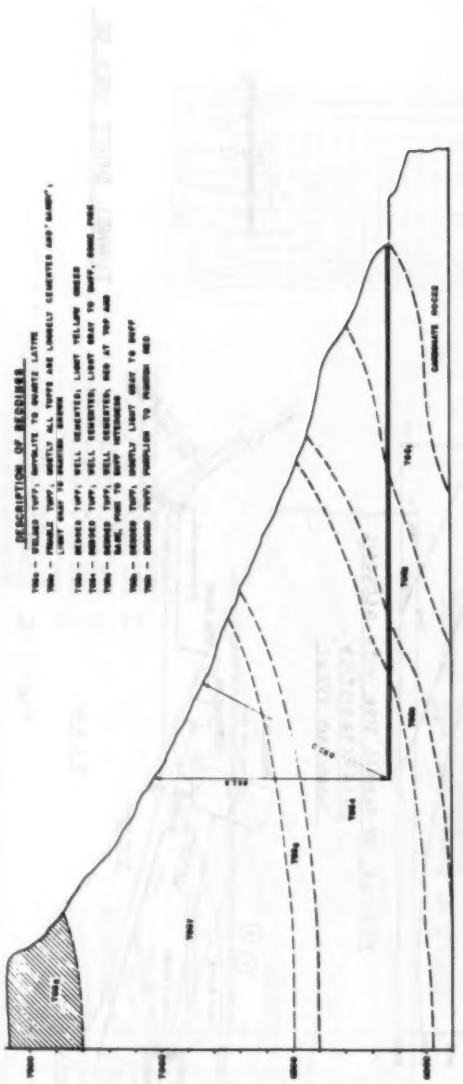

Figure 5A. Profile of Blanca site.

Figure 6A. Profile of Logan site.

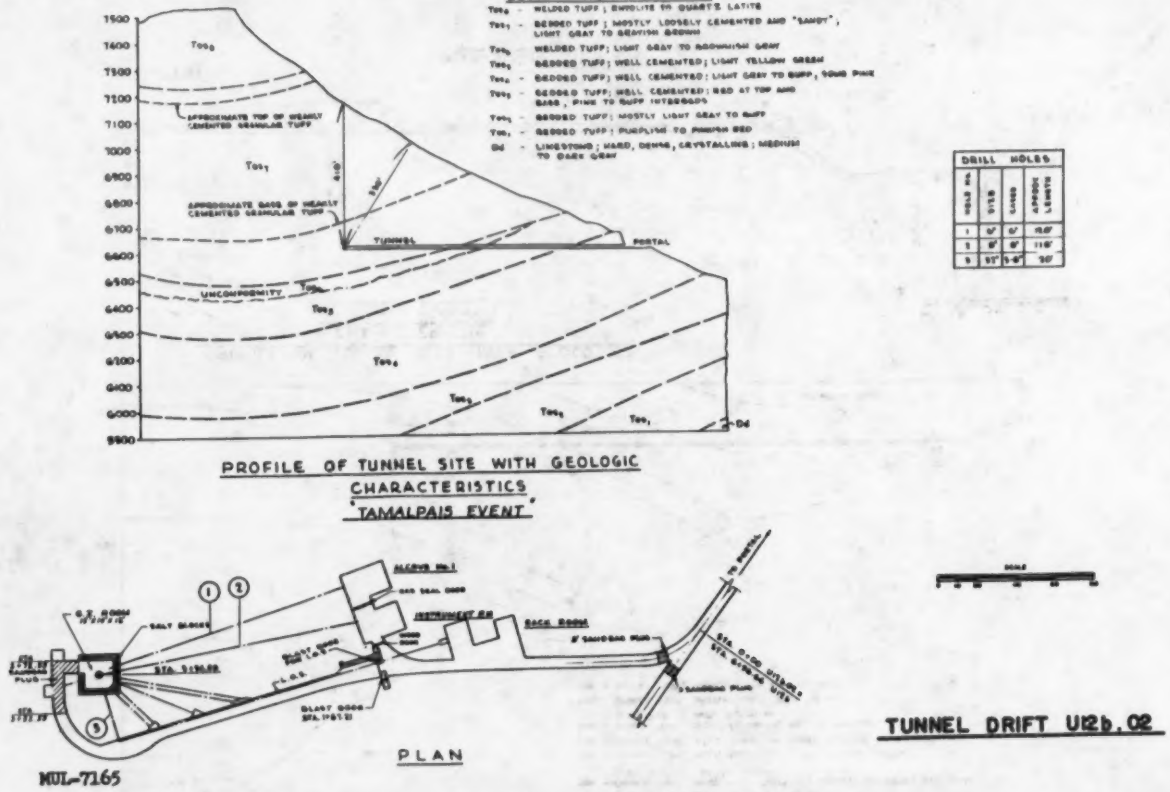
DESCRIPTION OF BEDDINGS
Tov6 - WELDED TUFF; DEVITRIFIED TO QUARTZ LATITE
Tov5 - BEDDED TUFF; MOSTLY LOOSELY CEMENTED AND "SANDY", LIGHT GRAY TO BROWNISH GRAY
Tov4 - WELDED TUFF; LIGHT GRAY TO REDDISH GRAY
Tov3 - BEDDED TUFF; WELL CEMENTED; LIGHT YELLOW GREEN
Tov2 - BEDDED TUFF; WELL CEMENTED; LIGHT GRAY TO BUFF, YOUNG PINK
Tov1 - BEDDED TUFF; WELL CEMENTED; RED AT TOP AND BASE, PINK TO BUFF INTERBEDS
Tov0 - BEDDED TUFF; MOSTLY LIGHT GRAY TO BUFF
Tob - BEDDED TUFF; PURPLISH TO MAROON RED
Od - LIMESTONE; HARD, DENSE, CRYSTALLINE; MEDIUM TO DARK GRAY
DRILL HOLES
APPROXIMATE TOP OF WEAKLY CEMENTED GRANULAR TUFF
APPROXIMATE BASE OF WEAKLY CEMENTED GRANULAR TUFF
TUNNEL
PORTAL
UNCONFORMITY
PROFILE OF TUNNEL SITE WITH GEOLOGIC CHARACTERISTICS TAMALPAIS EVENT
PLAN
TUNNEL DRIFT U12b.02
MUL-7165

Figure 7A. Profile of Tamalpais site.

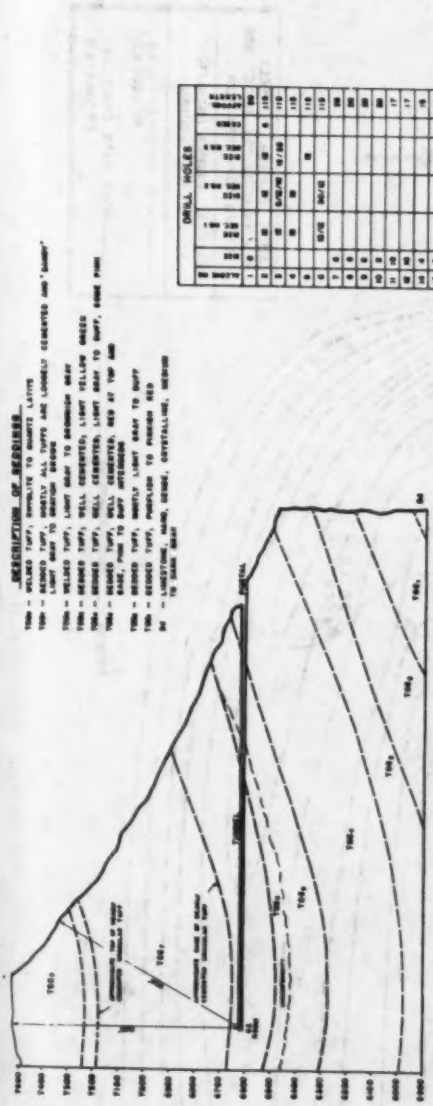

Figure 7A. Profile of Tamalpais site.

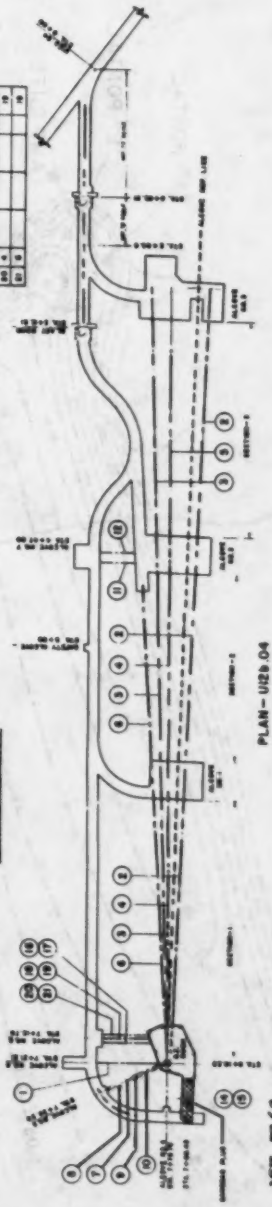

Figure 8A. Profile of Evans site.

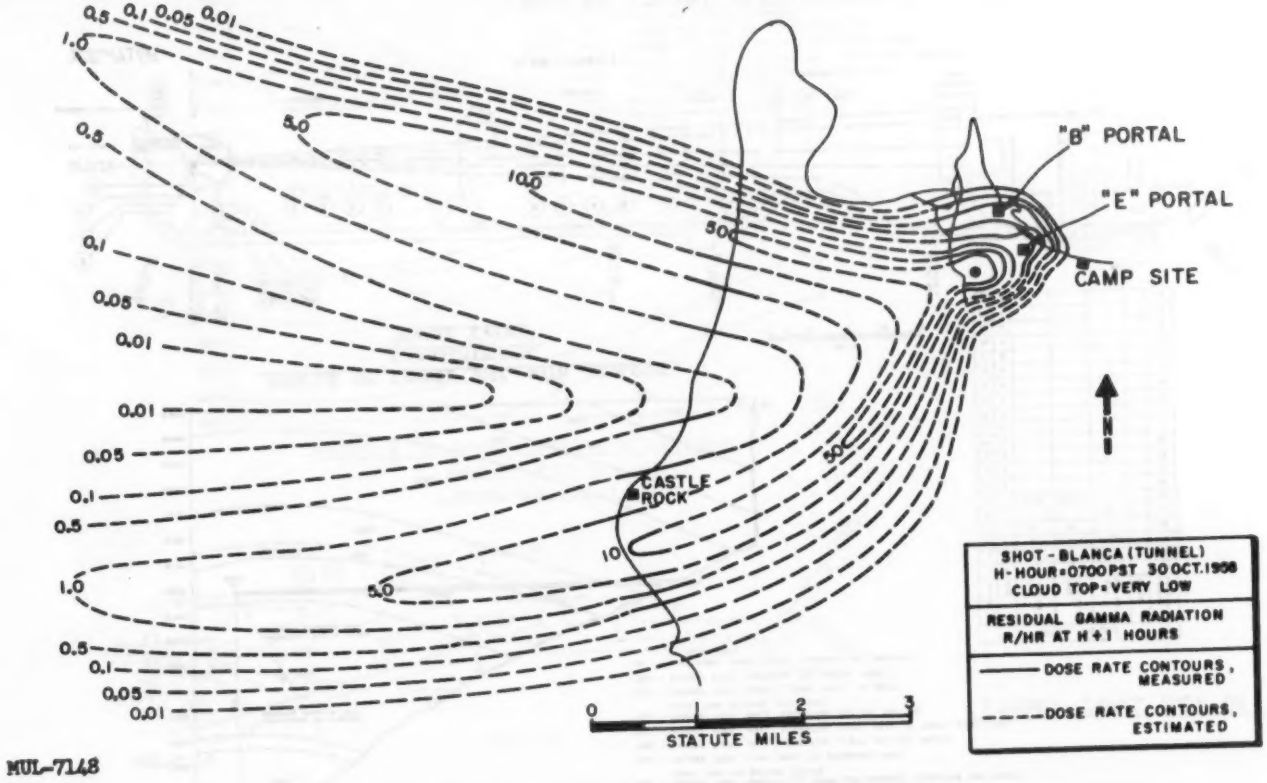

Figure 9A. Fallout pattern from Blanca Event.

Figure 9A. Fallout pattern from Blanca Event.

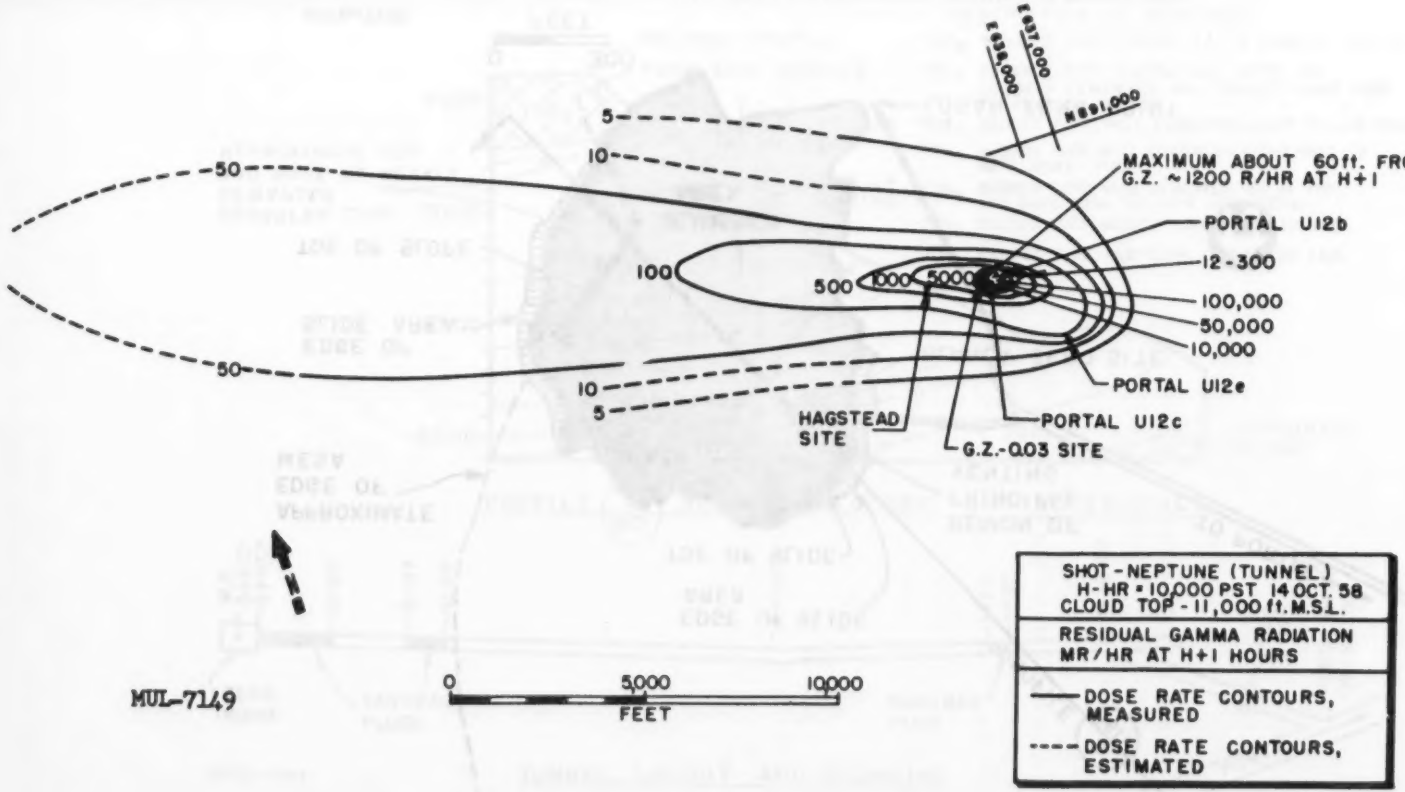

Figure 10A. Fallout pattern from Neptune Event.

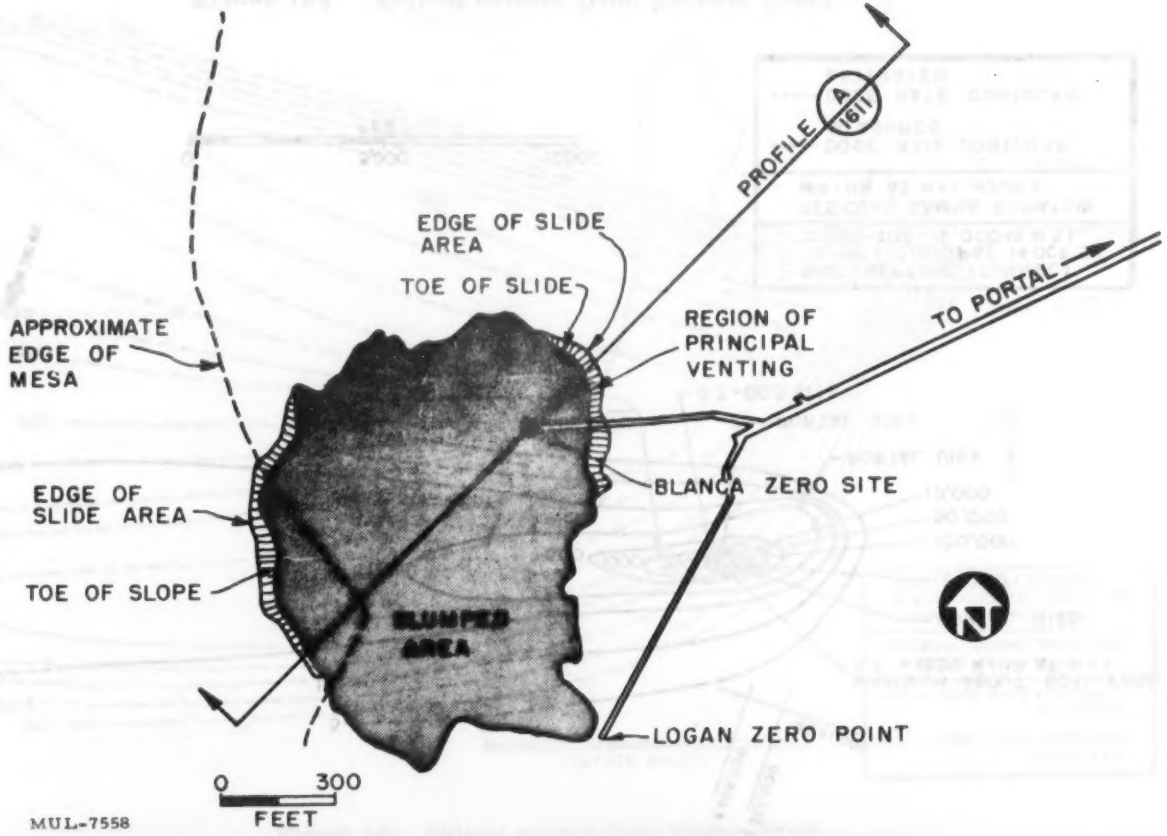

Figure 11A. Pre- and post-shot profiles of Blanca site.

DESCRIPTION OF BEDDINGS:

MUL-7558

FEET

Figure 11A. Pre- and post-shot profiles of Blanca site.

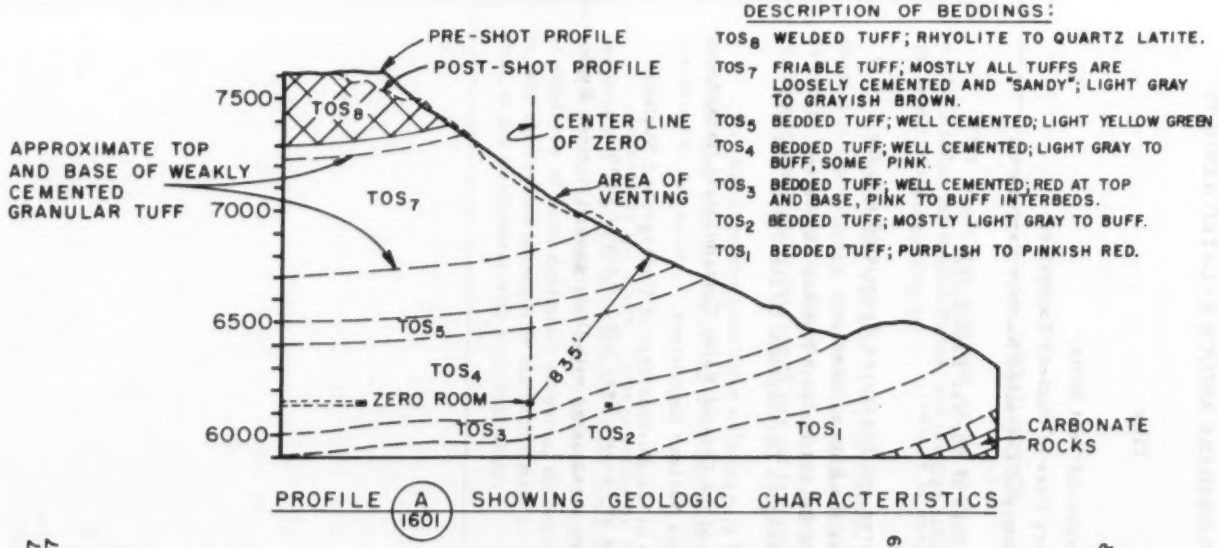

Figure 12A. Plan view of Blanca slumped area.

LIST OF PREVIOUS PLOWSHARE AND/OR RELATED REPORTS

	Title
UCRL-4659	Deep Underground Test Shots.
UCRL-5026	Non-Military Uses of Nuclear Explosions.
UCRL-5124 Rev. I	Phenomenology of Contained Nuclear Explosions.
UCRL-5253	Industrial Uses of Nuclear Explosives.
UCRL-5257 Rev.	Peaceful Uses of Fusion.
UCRL-5281	Temperatures and Pressures Associated with the Cavity Produced by the Rainier Event.
UCRL-5457	Large Scale Excavation with Nuclear Explosives.
UCRL-5458	Mineral Resource Development by the Use of Nuclear Explosives.
UCRL-5538	Evaluation of the Ground Water Contamination Hazard from Underground Nuclear Explosions.
UCRL-5542	Properties of the Environment of Underground Nuclear Detonations at Nevada Test Site. Rainier Event.
UCRL-5623	Radioactivity Associated with Underground Nuclear Explosions.

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Representative HOLIFIELD. Our next witness will be Dr. Richard M. Foose, chief of the department of earth sciences, Stanford Research Institute, and a member of Technical Working Group II of the Conference on the Suspension of Nuclear Tests. He will present testimony on procedures and techniques of on-site inspection to include team phasing, individual techniques, and future programs and requirements.

He received his Ph. D. in structural and economic geology at the Johns Hopkins University in 1942.

You may proceed, Dr. Foose.

STATEMENT OF RICHARD M. FOOSE,[1] CHIEF, DEPARTMENT OF EARTH SCIENCES, STANFORD RESEARCH INSTITUTE

Dr. FOOSE. Mr. Chairman and members of the committee, this presentation will be concerned mainly with three subjects: the procedures that an inspection group might follow, the instrumental and other techniques that could be used by the group, and research programs that might improve inspection capability.

At the beginning, I would like to emphasize that even if it were known that a nuclear explosion had occurred, if through decoupling or any other means a seismic signal were not received, then there would be no basis for inspection as the present system is conceived.

On-site inspection will be required as an integral part of any control system for nuclear testing as long as instrumental techniques for detection of a seismic event are unable to identify the cause of that event and as long as physical evidence must be collected to provide proof that a nuclear explosion had occurred.

The capability of an inspection group to accurately identify a seismic event has been variously estimated by some to be zero, and by others to be 100 percent. This unusual range of opinion reflects the limited knowledge we have of techniques that might be employed by an inspection group, and even more important, the complete lack of experience in applying these techniques to either a real or simulated clandestine nuclear test.

Although it is not possible to state the degree of likelihood of accurate identification, generally there would always be some likelihood and, with increased experience in the application of inspection techniques, the likelihood might improve.

The only occasion on which techniques potentially applicable to onsite inspection have been tried in the field were during the Hardtack experiments in October 1958, and during the Cowboy experiments recently concluded in Louisiana.

The final report—WT–1739—describing investigations made at Hardtack, titled "Identification of Possible Underground Nuclear Explosions by Onsite Inspection," by Richard M. Foose and Robert B.

[1] Head of the earth sciences department, division of physical and biological sciences, Stanford Research Institute, Menlo Park, Calif.

From 1937 to 1939 he was an assistant instructor in geology at Northwestern University.

From 1939 till 1946 he was on the staff of the Pennsylvania Geological Survey, the last 3 years as senior geologist in charge of the strategic minerals program.

In 1946 he went to Franklin and Marshall College to organize a department of geology and was professor and head of that department till 1957 when he went to Stanford Research Institute to organize the department of earth sciences.

He was in charge of the research studies dealing with onsite inspection techniques carried out during the Hardtack experiments, October 1958, and the Cowboy experiments, January–March 1960.

In November–December 1959 he was one of the scientists who participated in Technical Working Group No. 2 of the conference on the Discontinuance of Nuclear Weapons Tests at Geneva.

Hoy discusses the practical application of aerial observation and photo-reconnaissance methods, as well as ground and underground inspection techniques.

The report attempts to evaluate the applicability and limitations of inspection methods, considers the role and operating methods of mobile inspection groups, tabulates clues that might be associated with the human activity peculiar to conducting underground nuclear explosions, and discusses the methods and equipment to be used in drilling to obtain a radiochemical sample. There is no report available as yet describing similar studies carried out during the Cowboy experiments.

This discussion of procedures and techniques of onsite inspection is based upon the work carried out during both Hardtack and Cowboy and upon theoretical considerations and discussions during the past 2 years.

INSPECTION GROUP PROCEDURE

The objective of an inspection group would be to (1) determine in the field the cause of a seismic event; and (2) to obtain proof it the event was caused by a nuclear explosion.

The initial task of the inspection group would be to reduce the size of the inspection area from the 100 to 500 square kilometers indicated by the teleseismic net to almost the exact epicenter so that proof of a nuclear event could be obtained by drilling for a radioactive sample.

Mr. Chairman, we have had various sizes of the original inspection area indicated by those who have testified previously; 100 to 500 square kilometers equates with 40 to 200 square miles.

Representative HOLIFIELD. How much was that?

Dr. FOOSE. About 40 to 200 square miles. If there is any deviation from this I would defer to Dr. Romney, who I think agrees that this is an appropriate figure.

If the evidence points to a natural earth shock as the cause of the detected seismic disturbance, the precision of this epicentral determination need not have a high degree of accuracy. In fact, it may be impossible to determine the epicentral location of an earthquake closer than the teleseismic net did originally.

In this case the primary objective of the inspection group will be achieved by positively identifying the event as an earthquake without the necessity of pinpointing the exact epicenter. However, if the event cannot be established definitely as a natural seismic event and suspicious circumstances point to a possible clandestine nuclear explosion, more precise epicentral locations will be required.

In approaching this task, the following three inspection phases would probably be used in the order indicated, although this might vary, depending upon individual situations.

Phase 1: Aerial reconnaissance—observation and photography—would be used to select one or more areas probably as small as 1 to 5 square kilometers. In the aerial phase of onsite inspection, most techniques are reconnaissance in nature; some are detailed. They include visual, photographic, radiation, magnetic, electromagnetic, and infrared surveys.

These surveys utilize all types and sizes of aircraft ranging from small helicopters to multiengine fixed-wing craft. The observational or survey altitudes range from 50 to 6,000 meters above the terrain.

Selection of type of aircraft and of survey elevation depends upon the environment to be inspected and the survey method being employed.

Phase 2: Detailed inspection by air, on the ground, underground, and on and under water would be made with the objective of selecting drilling sites within 500 feet of the epicenter. The detailed techniques of inspection include both direct visual examination and sophisticated and complex geophysical techniques, such as seismic profiling.

Phase 3: Drilling with the objective of penetrating the explosion site and collecting a radiochemical sample. The drilling phase is mechanical in nature and includes very well developed techniques.

In carrying out phase 1, air reconnaissance, four alternative findings may result, and in each of these cases alternative decisions are possible:

1. Terrain and cultural evidence might definitely indicate the seismic event was an earthquake. The decision alternative is to (1) terminate inspection without any ground examination; or to (2) carry out routine and corroborative ground examination.

2. Terrain and cultural evidence definitely indicate seismic event was an underground explosion. The decision alternative is to (1) terminate the inspection in the rare event that radiochemical proof might be obtained from air—due to venting; or to (2) carry out inspection on the ground of a well-defined target area.

3. There is no terrain or cultural evidence that would strongly indicate seismic cause, but there is evidence of human activity within the area that might be associated with an explosive experiment. The decision alternative is to (1) terminate inspection; or (2) to carry out ground examination in one or several areas of 1 to 5 square miles.

4. There is no terrain or cultural evidence that would indicate seismic cause and no evidence of human activity. The decision alternative is (1) to terminate inspection; or (2) to carry out inspection on the surface by either (*a*) one or two traverses across target area; or (*b*) by one or more spot checks within the target area.

In carrying out phase 2 on the ground, visual inspection would be made in areas of probably 1 to 5 square miles. The alternatives and possible decisions that exist after inspection of several hours to 1-day duration are that:

1. The evidence indicates seismic event was an earthquake in which case the inspection could be terminated.

2. The evidence strongly suggests the seismic event was an underground explosion, in which case appropriate detailed inspection techniques would be used to define the epicentral area. The techniques used first in most cases would be low-level aerial observation and large-scale photography from the air and aerial infrared surveying. On the ground they would probably be seismic noise monitoring and vertical motion surveying.

3. The evidence is inconclusive or suggests "no event," in which case the inspection would be (1) terminated; or (2) some or all of above detailed inspection techniques would be used as a further check.

The detailed inspection technqiues in phase 2 would be used in areas of probably one-half to 1 square mile with the objective of defining the epicentral area to less than a radius of 500 feet.

In phase 3, drilling would be carried out with the objective of collecting a radiochemical sample that would constitute proof of a nuclear test. Assuming that the epicentral area had been defined in the prior phase of inspection to an area 500 feet in radius, the follow-

ing table indicates the number of drill holes in a triangular grid pattern that would be required to insure 100 percent completeness of search for a radioactive zone of specified radius at a depth that would insure no surface disturbance.

Yield	Containment depth (feet)	Approximate radius of radioactive zone (feet)	Number of holes	Total drill footage
1.7 kilotons	1,200	60	63	75,600
20 kilotons	2,700	150	10	27,000
100 kilotons	4,700	250	4	18,800

The estimated time in days and the cost to drill the required footage at the rate of 25 feet per shift is indicated below:

Footage	Estimated cost [1]	1 shift	Time (days) 3 shifts	15 shifts
75,600	$378,000	$3,024	1,008	202
27,000	135,000	1,080	360	72
18,800	94,000	752	251	50

[1] At $5 per foot and exclusive of all mobilization costs.

This assumes that all holes were drilled and that you might not find it until the last hole.

Assuming that only a specified number of holes, n, were drilled in a square grid pattern within an epicentral area of 500 feet radius, the probability of discovery—in percent—of the radioactive zone is indicated below:

Yield	5 holes	10 holes
1.7 kilotons	0.03	0.12
20 kilotons	.26	.94
100 kilotons	1.00	1.00

Thus, five drill holes would provide a 26-percent chance of discovering a 20-kiloton size zone in a 500-foot radius epicentral area.

INSPECTION TECHNIQUES

The effectiveness of an inspection group would be dependent ultimately upon the capability of the individual techniques and of the group to use them with a high level of imagination.

Techniques that might be useful fall into four main classes: (1) Visual and photographic; (2) surveying; (3) geophysical; and (4) geochemical. All but the first involve instrumental measurements.

The only attempts to evaluate the applicability of these techniques to problems of onsite inspection in the field have been during the Hardtack II and Cowboy experimental tests. The techniques used include visual examination, photography, and radiation surveys in the air, on the surface, and underground; and seismic noise monitoring and vertical displacement surveys on the surface.

Limited observations were also made of the techniques of seismic profiling and water analysis. The geophysical techniques involving magnetic, electrical, electromagnetic, and gravity phenomena were not studied at Hardtack or Cowboy. They have been used extensively in the field for other purposes, however, and have received serious consideration by experts regarding their possible application to inspection.

Since all measurements and observations on these techniques at Hardtack and Cowboy were made with precise knowledge of the location of the explosive device and all of the important characteristics of the environment were studied both before and after the detonation, any evaluation of the techniques must be considered in the context of no concealment.

Representative HOLIFIELD. You mean by that that you would have to approach the problem completely different from your actions at Hardtack and Cowboy because you had all the pertinent facts?

Dr. FOOSE. That is correct.

Representative HOLIFIELD. You did not have to ascertain them.

Dr. FOOSE. That is correct.

Representative HOLIFIELD. Therefore, the only thing that we have in the Hardtack and Cowboy series is the experience of the registering of instruments and the time for drilling in, the time for constructing the holes.

Dr. FOOSE. And the visual observations.

Representative HOLIFIELD. And the visual observations that were involved.

Dr. FOOSE. This was all carried out with full knowledge of where the device was and knowledge of the environment before and afterwards. I am saying, therefore, that an effective evaluation cannot be made without an attempt to carry out a simulated or a real clandestine nuclear test. Until that time our evaluations will be based upon the full knowledge of these things.

Representative DURHAM. If you had about 10 seismic disturbances and had to inspect them on the basis of what you told us here, you would probably have full employment in most of the country if we tried to find them.

Representative VAN ZANDT. When Mr. Foose was introduced, I identified him immediately as a Pennsylvania Dutchman.

Dr. FOOSE. That is correct.

Representative VAN ZANDT. I see you are from Lancaster, Pa.; therefore, you are no doubt familiar with Pennsylvania's mining activities, both anthracite and bituminous?

Dr. FOOSE. Very familiar.

Representative VAN ZANDT. What about the utilization of our abandoned coal mines since, as you know, we have hundreds of them which include a lot of deep mines. Could they be used for this experiment?

Dr. FOOSE. Mines could certainly be used for the experiment, Mr. Van Zandt. In fact, probably mines would be one of the appropriate places for carrying out an experiment of this type.

Representative VAN ZANDT. I notice here in your statement that you talk about the central control and tunnels leading out to test sites. That is typical of what you find in a lot of these coal mines. They have a central control point with tunnels leading out to the

pockets of coal. To me it would be a natural and save the Government millions of dollars.

Dr. FOOSE. It is probably also true, reflecting Dr. Violet's testimony, that there would have to be special attention paid to the preparation of the mine for carrying out the test. You see, the Hartack experiment really was testing in a mine, but in this case a mine created for that purpose.

Representative VAN ZANDT. What percentage of overall cost would you charge to actually the drilling of the central control point and tunnels?

Dr. FOOSE. If you are asking about the planning and the execution of the test, then I am not capable of answering that.

Representative VAN ZANDT. Let us take the overall preparation of a mine for the purpose of testing. What percentage of it would you charge of to actually preparing the mine, such as tunneling, and so forth?

Dr. FOOSE. I think the question should be answered, Mr. Van Zandt, by those who have actually carried out testing and have, therefore, the figures.

Representative HOLIFIELD. We do have some figures on the tunneling. I think the estimate was around $13 million for a certain type of exploration by one of the previous witnesses.

Representative VAN ZANDT. Percentagewise, does it represent 75 percent of the overall cost? That is what I am trying to arrive at.

Dr. FOOSE. I am really indicating that I am not competent to answer. I just do not know. I have not given that any attention.

Representative HOLIFIELD. Dr. Beers may be able to answer that and he is the next witness.

Pursuing this matter that I think Mr. Van Zandt so appropriately brought up, we have many thousands of miles of mining tunnels in the United States of different kinds—coal mining, silver, lead, gold, zinc, all these types.

We are all aware of communities such as Joplin, Mo., where there is a complete labyrinth of underground tunnels and cavities. I am sure in the area of Pittsburgh, Mr. Van Zandt, it is the same way. If you wanted to set off a test without showing outside extracurricular activity, you might say, why would it not be possible to use some of these tunnels, dig out the size chamber you needed by high-explosive methods and store the debris in other unused tunnels and go right ahead and have the testing in these chambers?

Dr. FOOSE. Mr. Chairman, I think there is no question about the fact that mines might be used for carrying out tests of this kind. I would point out that in doing this it will be essential that the shot be contained just as effectively from surface disturbance of the tunnels as from surface disturbance of the earth's surface, because an inspection team would be able to move into the mine just as well as they can move across the surface.

If there is obvious disturbance, noncharacteristic of normal mining activities within a mine tunnel, this would, of course, attract the attention of an inspection group.

Representative HOLIFIELD. But this would presuppose the determination that a suspicious event had occurred.

Dr. FOOSE. Of course.

Representative HOLIFIELD. If it occurred in a comparatively large chamber underground, and there are many of those large caverns underground, then the decoupling effect might preclude the establishment of a suspicious event and, therefore, there would be no inspection called for.

Dr. FOOSE. You are quite right. As I said at the beginning, this entire discussion of how you would go about an on-site inspection is predicated upon the receipt of a signal indicating that there was an event that was not identified and, therefore, was subject to being selected as an inspection case.

Representative HOLIFIELD. It is quite well known that the Russians have extensive mining operations, is it not?

Dr. FOOSE. That is right.

Representative HOLIFIELD. Our Bettis Laboratory near Pittsburgh happens to be over an abandoned coal mine, I understand.

Representative BATES. Dr. Foose, how do you definitely determine the seismic event was an earthquake?

Dr. FOOSE. Mr. Bates, we have gotten back into testimony that pertains to detection rather than inspection.

Representative BATES. I mean from your statement here. You indicate that under these different situations the evidence indicates that the seismic event was an earthquake.

Dr. FOOSE. Yes; I am sorry.

Representative BATES. With respect to that portion of your statement, how do you definitely determine it was that?

Dr. FOOSE. One of the alternatives would be that the inspection definitely would indicate that it was an earthquake. Some of the things that would definitely indicate that it was an earthquake would be terrain disturbances over a large area, much larger than could be occasioned by the detonation of an explosive.

Representative BATES. Much larger than a nuclear explosion?

Dr. FOOSE. Much larger than a nuclear explosion; that is correct. An earthquake usually has the characteristics, if there are terrain effects at all, of having these terrain effects over a much larger area than would be the case with any kind of explosive—high explosive or nuclear explosive.

For instance, the recent effects of the earthquake in Yellowstone Park last year were widespread and could not in any way have been confused by an inspection team as having been caused by a nuclear explosion.

Mr. CONWAY. But these would give seismic reading that your instruments would indicate even before you went out to inspect on-site.

Dr. FOOSE. Possibly so. But if they were unidentified by the seismic detection net and if an inspection team went in and found these evidences on the surface, then there would be no question that an earthquake was the cause.

Representative HOLIFIELD. In some of the large earthquakes, as I understand it, the area where the initial movement starts is spread over many, maybe many miles rather than being in a solid location where a nuclear event might occur.

Dr. FOOSE. That is correct. Most earthquakes do involve a slippage of a part of the crust. If it is of sufficient magnitude, this almost automatically means a rather widespread terrain effect. If the earthquake is of a smaller magnitude there may be no terrain effect.

Representative HOLIFIELD. Would you say a large earthquake has no epicenter, as we think of epicenter in the nuclear explosion?

Dr. FOOSE. Not as precise an epicenter, I think.

Representative HOLIFIELD. In other words, the starting area would be much larger than it would be with a nuclear explosion, comparatively speaking.

Dr. FOOSE. Yes.

Representative HOLIFIELD. Therefore, your signals in different stations would be different, would they not?

Dr. FOOSE. This is generally correct. In the case of an earthquake we must conceive of it as starting at a point, but very rapidly it may develop into an action over a very large extent of the earth's crust.

Along the San Andreas fault, there has been earthquake movement for hundreds of miles. The initial movement may have started at a point, but very quickly developed to a very great extent.

Representative HOLIFIELD. Almost the full length of the State of California. I hate to give this advertising.

Representative HOSMER. Northern California.

Representative HOLIFIELD. I can remember that Long Beach looked pretty sick when I went down to see it.

Representative HOSMER. That was a different domestic earthquake.

Representative BATES. How often would you have this type that would be readily recognizable as an earthquake? Is that an unusual event when it has this significant nature?

Dr. FOOSE. You mean where there would be terrain effects that would leave no doubt in a person's mind that this was in fact an earthquake? I cannot give you a number.

Representative BATES. Is it very rare?

Dr. FOOSE. It is not very rare, but it is not very common either. I just cannot apply an exact number to it. Certainly in the United States it would be something less than five in a couple of years.

Representative BATES. In seismic reading, what would that be approximately?

Dr. FOOSE. This is one of the areas of research that is definitely needed. We need to be able to equate the kinds of terrain effects and the area of terrain effects to magnitudes.

Representative BATES. It would be over the 4.75 reading?

Dr. FOOSE. I would say in almost every case, yes. But I am not sure it would be in every case.

Representative VAN ZANDT. Doctor, was the signal from the Yellowstone Park earthquake recorded worldwide or was it in a limited area?

Dr. FOOSE. It was recorded for very great distances. I cannot answer your question specifically. I am sure Dr. Romney or one of the other seismologists in the room could. Not being a seismologist, I really do not know that. It was recorded over very large distances.

Representative HOLIFIELD. You may proceed.

Dr. FOOSE. Experience at Hardtack:

At Hardtack all of the underground shots had some surface expression. Blanca, Logan, and Neptune shots had unmistakable surface evidence in the form of fractures, rock displacement and vegetative disruption visible both by direct observation from high altitudes and on 1:20,000-scale aerial photographs.

The earlier Rainier shot also was clearly identified by observation and aerial photography of altered terrain features even a year afterward. Tamalpais and Evans shots were not identified from the air.

Representative BATES. I wonder if you could go back to the question I asked originally, determining from the surface whether this was actually an earthquake. I gather from what you had said previously that the nuclear shot would not give you a disturbance on the surface in the form of fractures, rock displacements, et cetera.

Dr. FOOSE. No, that is a misunderstanding. What I mean to say and hope to make clear is that it would be quite easy if there were terrain effects from an earthquake and from an explosion to distinguish between them because of the wider spread of the effects from the earthquake and the more precise and centralized effects caused by the explosion.

Representative BATES. So it could be just over a broader area but the image itself could be similar in a narrow band.

Dr. FOOSE. It could be similar, but there are also very diagnostic characteristics that would be associated with the effects from an earthquake and those from a nuclear explosion.

For instance, the effects I am now describing from Blanca, Neptune, and others, were not only centralized, but had certain orientations to the fractures that were created by the explosion which would not be the same kind of orientation, for instance, that is associated with an earthquake.

So there are other diagnostic differences that one would be able to quickly identify.

Aside from the direct evidence of altered terrain features, the evidence of human activity, particularly that of many concentrating roads, served to define the Hardtack area very clearly.

All of the Hardtack shots could have been planned and executed at a depth sufficient to insure containment and, hence, no physical alteration of the surface. Also, the specific human activity associated with a nuclear shot might be camouflaged, but elimination of all evidence of human activity—and of general types of activity—would be extraordinarily difficult.

Hence, aerial observation and photography would be a highly important inspection technique for more precisely defining areas for detailed examination.

Representative HOLIFIELD. You are talking about the preparation of the area for the test there, are you not?

Dr. FOOSE. Exactly. I am talking about the kinds of human activity that would be associated with test activity.

Representative HOLIFIELD. If this was handled in an underground mine, as Mr. Van Zandt suggested, you would have your normal activity, the mining operation, the dumping of debris and that sort of thing.

Dr. FOOSE. This is true, but a very important point, Mr. Chairman, is that if we were going into an inspection area of a hundred square miles and if we found in that area one mine and literally no other human activity, or only an innocuous kind of human activity, we would go to that mine first of all. This would serve, then, to define the 1 to 5 square miles for priority inspection effort, even though all we saw from the air was mining activity.

Representative HOSMER. Even if you came in there and found that there was an explosion, then the host country says, "Well, I will

be darned, some dirty foreigners must have come in there and blew off a bomb." You wouldn't have much chance of finding out who actually did blow it off, would you, under those circumstances?

Dr. FOOSE. Thus far I am confining my remarks to the technical approach that would be made by the inspection team.

Representative HOSMER. That would probably be the anticipated consequence of discovering something going off like that. I suppose the inspection team ought to be prepared to go into that, too.

Dr. FOOSE. I think you are probably right that there are aspects of inspection that may supersede, in some cases, the technical function of the group. They must be prepared, perhaps, to devote some of their attention to the psychological, even the philosophical aspects of inspection.

Representative HOLIFIELD. Will you proceed, please.

Dr. FOOSE. Visual examination on the ground and underground of both terrain features and the features associated with human activity at Hardtack served to define the epicenter of all shots to within a radius of 100 feet with the possible exception of Tamalpais. In an appendix (No. 1, p. 301) attached to this statement some of the clues that might be observed during examination of an area are listed.

Following the Evans, Logan, and Blanca shots, attempts were made to listen to aftershocks with geophones with the objective of defining the disturbed region around the detonation point. Aftershocks were recorded, but the equipment was operated for such a limited period of time that the technique still remains to be evaluated for both explosive and earthquake seismic events.

Vertical displacement, occasioned by subsidence of the surface following Blanca, was measured over a period of 90 days by the use of liquid-level gages. If a shot were not so deep that surface subsidence were precluded, this technique is capable of accurately measuring changes in surface elevation and of closely defining the epicenter.

While this summarizes the results and the extent of actual field study of possible inspection techniques associated with actual nuclear tests, there has been considerable discussion of other techniques that might be applicable to the problem of on-site inspection. In an appendix (No. 2, p. 302) attached to this statement, the principle of operation, the applicability, and the limitations of the techniques are considered more fully. These are briefly summarized in table 1 (p. 300) to show in which phase of the inspection procedure they would be used, the environment in which they can be used, and the approximate precision of location of the seismic epicenter that might be expected from their use.

In table 1 you will see the techniques listed in the first column. The second column indicates the phase of inspection in which they would most likely find their use. Some are used in all phases of inspection. The third column indicates the environment in which they would be used—in the air, on the ground, under ground, on or under water, or in the drilling operation. The fourth column attempts to indicate very approximately the precision of location of the epicenter in feet that might be expected from the use of these techniques. You will see that if the techniques are applicable, most of them expect to indicate the epicenter to within hundreds of feet.

Future research programs: It is obvious that the state of the art of on-site inspection is not very far advanced. It could be materially improved by carrying out the following research programs.

First, there is a critical need to establish the highest possible level of knowledge about each of the inspection techniques that have been considered thus far. By appropriate consultation with experts and a careful analysis of the records and the literature for each technique, a syllabus should be prepared that will fully describe the use and potential application of each of the techniques to on-site inspection.

Second, those techniques that have already had their value demonstrated should be further evaluated and improved under test conditions that are as realistic as possible.

Third, those techniques that appear to offer promise should be critically tested under the same realistic field conditions to establish their degree of applicability.

Fourth, the research programs should be designed to test the effectiveness of all the techniques under a wide variable set of environmental conditions. One of the major environmental variations is that of continental versus marine (including inland water bodies) locations. Other major environmental variables are climate, topography, vegetation, the type of rocks (lithology), and the kinds of surface features that cover the bedrock (sand dunes, glaciers, etc.).

Until this time our experience has been almost solely limited to the Nevada test site, to the rock which we call tuff, to a climate which is semiarid, and to the topography represented by a mesa under which the tests have taken place. So we have an extremely limited appreciation of the environmental effects that might be faced. An effort should be made with planned experiments to compare the effectiveness of techniques against enough of these variables so that useful extrapolations can be made with a reasonable degree of confidence.

In an appendix (No. 3, p. 305) attached to this statement there is a summary of some of the specific research efforts that are suggested for the improvement of inspection techniques and procedures.

Representative DURHAM. Could you carry out this research program as outlined by you without actual testing?

Dr. FOOSE. Some of the research, sir, could be carried out without actual testing, yes. Much of it could not.

Representative DURHAM. What do you mean much of it could not? Fifty percent of it or 25 percent?

Dr. FOOSE. A rough order of magnitude would certainly indicate that we are talking about 60 to 80 percent of the research that would have to be geared to realistic testing. But a significant portion of research could be carried out without testing of any kind.

Representative HOLIFIELD. Dr. Foose, you have made a very orderly and valuable statement. I am sure members may have some questions. Congressman Hosmer?

Representative HOSMER. Could you tell me what would happen to your core drilling if the suspected area were an area in which there had been dumping of radioactive waste—high level—in an underground cavity?

Dr. FOOSE. Do you mean if we drilled into that area?

Representative HOSMER. Yes. I am trying to ascertain whether or not you could pull a little string by doing it in an area where you had deposited radioactive waste anyway.

Dr. FOOSE. You mean to spoof the radioactive analysis by use of waste instead of the radioactivity from the explosion?

Representative HOSMER. That is right.

Dr. FOOSE. Although this question can be answered more competently by another, I think the nature of the radioactive fission products are such that one would be very easily able to tell that it was not from the explosion and that it was dumped waste. I am fairly certain that this is correct.

Representative HOSMER. Then it would be advantageous to have that kind of radioactive waste in an area where you are going to attempt to screen a test so that if the inspection team came in, did drill and pull it up, they would just say this obviously is just radioactive waste, we will go home.

Dr. FOOSE. If they were very suspicious for other reasons, I don't think they would do just what you said; they would suspect that this was an attempt to spoof. If the signal was strong that sent the inspection team into the area and if other events brought them onto the ground, and if they were suspicious for one reason or another, then, I think, the team would not be fooled by this one.

Representative HOSMER. Thank you.

Representative BATES. Doctor, you indicated containment depths of several hundred feet, one of which was the yield of 20 kilotons at a containment depth of 2,700 feet. A little while ago we got a figure that 20 kilotons was 1,080 feet. Is your adjustment an approximation or just used as an example or what, or does it have a direct relation to the suppression of the radioactivity above the surface?

Dr. FOOSE. I think this is geared directly to the testimony of Dr. Johnson, Mr. Bates. I believe he also lists 2,700 feet.

Representative BATES. I thought I remembered that, 20 kilotons at 1,080 feet. One kiloton would require 400 feet of overburden, 20 kilotons, 1,080.

Dr. FOOSE. Twenty kilotons is 2,700 feet.

They are talking about complete containment of the radioactivity.

Representative BATES. That is what I am talking about.

Dr. FOOSE. However, I am talking about complete containment that would prevent any surface expression or terrain alteration of any kind. In other words, one could contain the radioactivity, as was done from Rainier, but in that case there was surface expression that was unmistakable, and an inspection would have had no difficulty whatever recognizing it.

Representative BATES. You are just talking about disturbances on the surface.

Dr. FOOSE. That is right. And this would require 2,700 feet.

Representative HOLIFIELD. Dr. Foose, in your introduction you state there is no report available as yet describing similar studies carried out during the Cowboy experiments.

Dr. FOOSE. Yes. This is soon ready but it is not ready yet.

Representative HOLIFIELD. In your opinion, when the report on the Cowboy experiments comes out, would the results be applicable to the detection of nuclear explosions when the data was derived from chemical explosions?

Dr. FOOSE. The Cowboy experiment, Mr. Chairman, allowed us to evaluate, again in the context of no concealment, the human activity primarily. There were, of course, no terrain effects whatever. So the

evaluation at Cowboy was primarily in terms of visual observation and aerial reconnaissance techniques.

Representative HOLIFIELD. You describe three phases which would be required to pinpoint a clandestine nuclear shot. In your opinion, how long would it take from the initial detection through phases 1, 2, and 3 to the final identification of a clandestine shot?

Dr. FOOSE. That is a very difficult question to answer. Let me try to do it, however, seriatim. Phase 1, I think under ideal conditions would allow an inspection team to get on to the ground within a period of 1 day. They would move into one or more areas that had been selected—of perhaps 1 to 5 square miles—as being the areas needing ground inspection.

Phase 2 is where it is extremely difficult to make an estimate because we don't know what we are going to find. If there was not complete containment, for instance, and if there was evidence right at the surface and this were rather well defined, we would be ready to move to phase 3 and begin drilling the next day. I think I have indicated that within the period of about 1 day on the ground we would be in a position of knowing what next to do; that is, whether to start the use of such techniques as vertical displacement surveys, seismic noise monitoring, and perhaps the application of magnetic and electromagnetic techniques to pick up artifacts, such as drill casing—and other materials that might have been left around as the result of the test emplacement.

To answer your question, when we come to phase 2, there is a wide range of time involved before one might be ready to go to phase 3, which is drilling.

Representative HOLIFIELD. Then on drilling you testified that might take a year, it might take a few months, according to the depth and the type of soil and so forth.

Dr. FOOSE. Yes. If we were within an epicentral area of 500 feet radius, for complete search for the 20 kiloton explosive, this would involve the drilling of 10 holes. We could drill those 10 holes in as little as 72 days.

Representative HOLIFIELD. This would assume that you would have the drilling equipment available, the expert people to do the drilling, and have the foundations poured for your drillings and all that sort of thing, I suppose.

Dr. FOOSE. If we got to the point where we were quite confident of having chosen the proper epicentral area of 500 feet radius, or perhaps less than that, my personal concept is that at this point we would bring in the drilling people for the first time with the mobile equipment and drills.

Representative HOLIFIELD. I can foresee from a practical standpoint, knowing something about drilling oil wells, as does Mr. Hosmer—he and I both have oil fields in our district—that the practical problem of bringing in skilled drilling crews and setting up your rigs and making all the preliminary moves that have to be made, would require an assumption of a terrifically competent and standing-ready organization in order to accomplish anything near what you say there.

Representative HOSMER. There is the problem of getting people and things from one point to another in this area that we would run up against.

Dr. FOOSE. The whole concept of inspection groups moving into an area does involve a state of readiness, with specialized personnel capable of handling each of these techniques, and in each of the successive phases. It certainly involves that concept. I do, however, want to correct an impression which I believe both of you California representatives have regarding the drilling equipment.

I think both of you are thinking of oil well drilling equipment, and this is not what would be used. One would use diamond drilling equipment, some of which is very portable in nature, some of which can be back-packed in, and has been many times. Portable drilling equipment that can be back-packed can drill to depths of 1,000 feet.

Something less portable than this that could easily be lifted into position by a small helicopter is capable of drilling to 3,000 feet. Mobilization time for this, assuming a readiness state at some central point, could be within a couple of days.

Representative HOLIFIELD. You make it sound very simple. I am acquainted with all kinds of well drilling, water drilling, water well drilling, cable tool drilling, rotary-type drilling, and I have had oil wells unfortunately drilled near my own property in California, but not on my property, both water wells and oil wells. I am not quite as optimistic about the time period as you are.

Representative HOSMER. As a matter of fact, you would probably have to go to drilling. You have listed four alternative situations that might occur from air reconnaissance. The first two have to do with finding cultural and terrain evidence of a violation if the clandestine test has been adequately carried on, you would almost automatically be in situations three and four.

Dr. FOOSE. That is right.

Representative HOSMER. So the time element in the discovery here would actually be governed by the drilling time.

Dr. FOOSE. You are right, we would be faced, I think, in most cases, assuming competency of concealment of the test, with situations three or four, assuming it is not an earthquake. We would not necessarily be involved in phase three, drilling, immediately. In fact, we almost certainly would be spending quite a little bit of time in the first two phases defining the epicentral area.

Representative HOSMER. So it is a year's job at best, isn't it?

Dr. FOOSE. Not necessarily, no. If we should be able to successfully define the epicenteral areas to tens of feet, the first drill hole might pick up the evidence quite readily.

For 20 kiloton shots, this would involve 2,700 feet of drilling. One hole could accomplish this—I mean one drill could accomplish this—on a three-shift basis in about a month.

Representative HOSMER. Is there any method when there are no surface manifestations that would allow you to define your epicenter that closely?

Dr. FOOSE. Yes, possibly so. The techniques involving magnetic and electromagnetic surveys, and the technique of vertical displacement surveys, if applicable, may succeed in defining the epicentral area quite precisely, to something certainly within tens of feet. If we are dealing with a radius of a radioactive zone of 150 feet, then it is quite likely that one drill hole might suffice to penetrate this zone. So it is not necessarily as grim a picture as requiring a year of time automatically, and the time can be shortened at any point in the

drilling operation by increasing the number of drills and the shifts of operation.

Representative BATES. How many people would you need to take care of your entire quota, if you had a quota for a year? It sounds like a rather large force if you are going to get into 15 shifts to work that 72-day period.

Dr. FOOSE. It would be quite large.

Representative BATES. If this is 1 location, if you have 30, where are all these people coming from? How many do you need for a shift?

Dr. FOOSE. For drilling, diamond drilling, you can drill adequately with three men on a shift.

Representative BATES. Three men on a shift?

Dr. FOOSE. That is right. If you had three shifts this would be 9 or 10 people. These would not be the only people involved in the team, but for the specific job of drilling this would be the number of people involved.

Representative BATES. I was just wondering, on page 5 on that 20 kiloton, where you have that 26-percent probability, with 10 holes, how do you get 94-percent probability?

Dr. FOOSE. This is based on a square grid pattern and by using that pattern in an epicentral area of 500 feet radius, we are just shy of 100 percent of complete saturation of the area with 10 holes. It is 94 percent. This is just a geometric operation of setting up the holes in a pattern and drilling down within this 500-feet radius area. Ten holes will result in 94 percent completness of search.

Representative BATES. I know that is what it says, but I was trying to figure out how that works into the grid.

Dr. FOOSE. Just draw the circle and then space the 10 holes in a rectangular grid pattern. In doing this, 10 holes will give just all but 100-percent coverage. With a triangular grid pattern, it gives us 100 percent.

Representative BATES. With half that number of holes, five, you get 26 percent?

Dr. FOOSE. That is correct. Because of the random placement of the holes. With only five holes, there is a 26-percent chance of drilling into the radius involving the radioactivity. But with 10 holes, the likelihood is up very sharply.

Representative HOSMER. I meant to ask you before, Dr. Foose, these figures you give us have to do with the Nevada formation. If the explosion was in granite, the figures would be different, would they not?

Dr. FOOSE. The figures would be different in the different media, that is correct.

Representative HOSMER. What media are you speaking of?

Dr. FOOSE. These figures are purposely geared to Dr. Johnson's paper. Dr. Johnson's paper is based upon the experience in Nevada tuff. There has been indicated already the possibility of the radius of the radioactive zone being somewhat less in more homogeneous and dense rock, such as granite, than it is in tuff. But as yet we have no experience about this. So we are basing our figures on the best evidence that is available, which is Nevada tuff.

Representative HOSMER. But it is quite logical to assume that the more homogeneous the medium, the more restricted will be the effects.

Dr. FOOSE. I would expect it to be somewhat more restricted.

Representative HOSMER. And the larger number of holes you would have to drill in order to meet the requirements of chance.

Dr. FOOSE. If the radius were less, it would require more holes, that is correct.

Representative HOLIFIELD. Thank you very much, sir. Your testimony will be a valuable addition to our compilation.

(The full statement of Dr. Foose follows:)

PROCEDURES AND TECHNIQUES OF ON-SITE INSPECTION

INTRODUCTION

On-site inspection will be required as an integral part of any control system for nuclear testing as long as instrumental techniques for detection of a seismic event are unable to identify the cause of that event and as long as physical evidence must be collected to provide proof that a nuclear explosion had occurred.

The capability of an inspection group to accurately identify a seismic event has been variously estimated by some to be zero and by others to be 100 percent. This unusual range of opinion reflects the limited knowledge we have of techniques that might be employed by an inspection group and, even more important, the complete lack of experience in applying these techniques to either a real or simulated clandestine nuclear test. Although it is not possible to state the degree of likelihood of accurate identification, generally there would always be some likelihood and, with increased experience in the application of inspection techniques, the likelihood might improve.

The only occasion on which techniques potentially applicable to on-site inspection have been tried in the field were during the Hardtack experiments, October 1958, and during the Cowboy experiments recently concluded in Louisiana. The final report (WT–1739) describing investigations made at Hardtack, titled "Identification of Possible Underground Nuclear Explosions by Onsite Inspection," discusses the practical application of aerial observation and photoreconnaissance methods as well as ground and underground inspection techniques. The report attempts to evaluate the applicability and limitations of inspection methods, considers the role and operating methods of mobile inspection groups, tabulates clues that might be associated with the human activity peculiar to conducting underground nuclear explosions, and discusses the methods and equipment to be used in drilling to obtain a radiochemical sample. There is no report available as yet describing similar studies carried out during the Cowboy experiments.

This discussion of procedures and techniques of on-site inspection is based upon the work carried out during both Hardtack and Cowboy and upon theoretical considerations and discussions during the past 2 years.

INSPECTION GROUP PROCEDURE

The objective of an inspection group would be to (1) determine in the field the cause of a seismic event, and (2) to obtain proof if the event was caused by a nuclear explosion.

The initial task of the inspection group would be to reduce the size of the inspection area from the 100 to 500 square kilometers indicated by the teleseismic net to almost the exact epicenter so that proof of a nuclear event could be obtained by drilling for a radioactive sample. If the evidence points to a natural earthshock as the cause of the detected seismic disturbance the precision of this epicentral determination need not have a high degree of accuracy. In fact, it may be impossible to determine the epicentral location of an earthquake closer than the teleseismic net did originally. In this case the primary objective of the inspection group will be achieved by positively identifying the event as an earthquake without the necessity of pinpointing the exact epicenter. However, if the event cannot be established definitely as a natural seismic event and suspicious circumstances point to a possible clandestine nuclear explosion, more precise epicentral locations will be required. In approaching this task, the following three inspection phases would probably be used in the order indicated, although this might vary, depending upon individual situations:

Phase 1. Aerial reconnaissance (observation and photography) would be used to select one or more areas probably as small as 1 to 5 square kilometers. In the

aerial phase on onsite inspection, most techniques are reconnaissance in nature; some are detailed. They include: visual, photographic, radiation, magnetic, electromagnetic, and infrared surveys. These surveys utilize all types and sizes of aircraft ranging from small helicopters to multiengine fixed wing craft. The observational or survey altitudes range from 50 to 6,000 meters above the terrain. Selection of type of aircraft and of survey elevation depends upon the environment to be inspected and the survey method being employed.

Phase 2. Detailed inspection by air, on the ground, underground, and on and under water would be made with the objective of selecting drilling sites within 500 feet of the epicenter. The detailed techniques of inspection include both direct visual examination and sophisticated and complex geophysical techniques, such as seismic profiling.

Phase 3. Drilling with the objective of penetrating the explosion site and collecting a radiochemical sample. The drilling phase is mechanical in nature and includes very well developed techniques.

In carrying out phase 1, air reconnaissance, four alternative findings may result and in each of these cases alternative decisions are possible:

1. Terrain and cultural evidence definitely indicate the seismic event was an earthquake. The decision alternative is to (1) terminate inspection without any ground examination, or to (2) carry out routine and corroborative ground examination.

2. Terrain and cultural evidence definitely indicate seismic event was an underground explosion. The decision alternative is to (1) terminate the inspection in the rare event that radiochemical proof might be obtained from air, due to venting, or to (2) carry out inspection on the ground of a well-defined target area.

3. There is no terrain or cultural evidence that would strongly indicate seismic cause, but there is evidence of human activity within the area that might be associated with an explosive experiment. The decision alternative is to (1) terminate inspection, or to (2) carry out ground examination in one or several areas of 1 to 5 square miles.

4. There is no terrain or cultural evidence that would indicate seismic cause and no evidence of human activity. The decision alternative is to (1) terminate inspection, or to (2) carry out inspection on the surface by either (a) one or two traverses across target area, or by (b) one or more spot checks within the target area.

In carrying out phase 2, on the ground, visual inspection would be made in areas of probably 1 to 5 square miles. The alternatives and possible decisions that exist after inspection of several hours to 1 day duration are that:

1. The evidence indicates seismic event was an earthquake, in which case the inspection could be terminated.

2. The evidence strongly suggests the seismic event was an underground explosion, in which case appropriate detailed inspection techniques would be used to define the epicentral area. The techniques used first in most cases would be low-level aerial observation and large-scale photography from the air and aerial infrared surveying; on the ground they would probably be seismic noise monitoring and vertical motion surveying.

3. The evidence is inconclusive or suggests "no event," in which case the inspection would be (1) terminated or (2) some or all of above detailed inspection techniques would be used as a further check.

The detailed inspection techniques in phase 2 would be used in areas of probably one-half to 1 square mile with the objective of defining the epicentral area to less than a radius of 500 feet.

In phase 3, drilling would be carried out with the objective of collecting a radiochemical sample that would constitute proof of a nuclear test. Assuming that the epicentral area had been defined in the prior phase of inspection to an area 500 feet in radius, the following table indicates the number of drill holes in a triangular grid pattern that would be required to insure 100 percent completeness of search for a radioactive zone of specified radius at a depth that would insure no surface disturbance.

Yield	Containment depth (feet)	Approximate radius of radioactive zone (feet)	Number of holes	Total drill footage
1.7 kilotons	1,200	60	63	75,600
20 kilotons	2,700	150	10	27,000
100 kilotons	4,700	250	4	18,800

The estimated time in days and the cost to drill the required footage at the rate of 25 feet per shift is indicated below.

Footage	Estimated cost [1]	1 shift	Time (days), 3 shifts	15 shifts
75,600	$378,000	$3,024	1,008	202
27,000	135,000	1,080	360	72
18,800	94,000	752	251	50

[1] At $5 per foot and exclusive of all mobilization costs.

Assuming that only a specified number of holes, n, were drilled in a square grid pattern within an epicentral area of 500 feet radius, the probability of discovery (in percent) of the radioactive zone is indicated below.

Yield	5 holes	10 holes
1.7 kilotons	0.03	0.12
20 kilotons	.26	.94
100 kilotons	1.00	1.00

Thus, five drill holes would provide a 26-percent chance of discovering a 20-kiloton-size zone in a 500-foot-radius epicentral area.

INSPECTION TECHNIQUES

The effectiveness of an inspection group would be dependent ultimately upon the capability of the individual techniques and of the group to use them with a high level of imagination.

Techniques that might be useful fall into four main classes: (1) Visual and photographic, (2) surveying, (3) geophysical, and (4) geochemical. All but the first involve instrumental measurements.

The only attempts to evaluate the applicability of these techniques to problems of onsite inspection in the field have been during the Hardtack II and Cowboy experimental tests. The techniques used include visual examination, photography, and radiation surveys, in the air, on the surface, and underground; and seismic noise monitoring and vertical displacement surveys on the surface. Limited observations were also made of the techniques of seismic profiling and water analysis. The geophysical techniques involving magnetic, electrical, electromagnetic, and gravity phenomena were not studied at Hardtack or Cowboy. They have been used extensively in the field for other purposes, however, and have received serious consideration by experts regarding their possible application to inspection.

Since all measurements and observations on these techniques at Hardtack and Cowboy were made with precise knowledge of the location of the explosive device and all of the important characteristics of the environment were studied both before and after the detonation, any evaluation of the techniques must be considered in the context of no concealment.

EXPERIENCE AT HARDTACK

At Hardtack all of the underground shots had some surface expression. Blanca, Logan, and Neptune shots had unmistakable surface evidence in the form of fractures, rock displacement and vegetative disruption visible both by direct observation from high altitudes and on 1:20,000-scale aerial photographs. The earlier Rainier shot also was clearly identified by observation and aerial photography of altered terrain features even a year afterward. Tamalpais and Evans shots were not identified from the air. Aside from the direct evidence of altered terrain features, the evidence of human activity, particularly that of many concentrating roads, served to define the Hardtack area very clearly.

All of the Hardtack shots could have been planned and executed at a depth sufficient to insure containment and, hence, no physical alteration of the surface. Also, the specific human activity associated with a nuclear shot might be camouflaged, but elimination of all evidence of human activity (and of general types of activity) would be extraordinarily difficult. Hence, aerial observation and photography would be highly important inspection techniques for more precisely defining areas for detailed examination.

Visual examination on the ground and underground of both terrain features and the features associated with human activity at Hardtack served to define the epicenter of all shots to within a radius of 100 feet with the possible exception of Tamalpais. In an appendix (No. 1) attached to this statement some of the clues that might be observed during examination of an area are listed.

Following the Evans, Logan, and Blanca shots, attempts were made to listen to aftershocks with geophones with the objective of defining the disturbed region around the detonation point. Aftershocks were recorded, but the equipment was operated for such a limited period of time that the technique still remains to be evaluated for both explosive and earthquake seismic events.

Vertical displacement, occasioned by subsidence of the surface following Blanca, was measured over a period of 90 days by the use of liquid-level gages. If a shot were not so deep that surface subsidence were precluded, this technique is capable of accurately measuring changes in surface elevation and of closely defining the epicenter.

While this summarizes the results and the extent of actual field study of possible inspection techniques associated with actual nuclear tests, there has been considerable discussion of other techniques that might be applicable to the problem of onsite inspection. In an appendix (No. 2) attached to this statement, the principle of operation, the applicability, and the linitations of the techniques are considered more fully. These are briefly summarized in table 1 to show in which phase of the inspection procedure they would be used, the environment in which they can be used, and the approximate precision of location of the seismic epicenter that might be expected from their use.

TABLE I.—*Summary of the applicability of on-site inspection techniques*

Technique	Phase of inspection [1]	Environmental use [2]	Approximate precision of location of epicenter (in feet)
Observation	R I D	A G U W D	Hundreds.
Photographic analysis	R I D	A G U W D	Do.
Radiation surveys	R I D	A G U D	Do.
Mensuration	I	G U	Tens.
Seismic noise monitoring	I	G U	Hundreds.
Seismic profiling	I	G W	Do.
Electrical surveys (electromagnetic)	R I D	A G U W D	Tens.
Magnetic surveys	R I	A G W	Do.
Infrared surveys	R I	A	Hundreds.
Acoustic surveys	R I	W	Do.
Neutron logging	D	D	
Geothermal logging	D	D	
Acoustical logging	D	D	Do.
Thermal gradient	I	U	
Geochemical analysis:			
Soil	I D	G U D	Do.
Water	I D	G U W D	Thousands.
Biogeochemical analysis	I	G	
Gravity surveys	None	A G U	

[1] R, reconnaissance (mostly air), phase 1; I, intermediate, phase 2; D, drilling, phase 3.
[2] A, air; G, ground; U, underground; W, water; D, drilling.

FUTURE RESEARCH PROGRAMS

It is obvious that the state of the art of on-site inspection is not very far advanced. It could be materially improved by carrying out the following research programs.

First, there is a critical need to establish the highest possible level of knowledge about each of the inspection techniques that have been considered thus far. By appropriate consultation with experts and a careful analysis of the records and the literature for each technique, a syllabus should be prepared that will fully describe the use and potential application of each of the techniques to on-site inspection.

Second, those techniques that have already had their value demonstrated should be further evaluated and improved under test conditions that are as realistic as possible.

Third, those techniques that appear to offer promise should be critically tested under the same realistic field conditions to establish their degree of applicability.

Fourth, the research programs should be designed to test the effectiveness of all the techniques under a wide variable set of environmental conditions. One of the major environmental variations is that of continental versus marine (including inland water bodies) locations. Other major environmental variables are climate, topography, vegetation, the type of rocks (lithology), and the kinds of surface features that cover the bedrock (sand dunes, glaciers, etc.). An effort should be made with planned experiments to compare the effectiveness of techniques against enough of these variables so that useful extrapolations can be made with a reasonable degree of confidence.

In an appendix (No. 3) attached to this statement there is a summary of some of the specific research efforts that are suggested for the improvement of inspection techniques and procedures.

Appendix No. 1. Checklist of Clues

I. Aerial observation and photo interpretation

A. Site preparation

1. *Vehicle.*—Heavily traveled roads ending in remote areas, camouflaged roads, dust on vegetation, parking lots, and cleared areas.
2. *Mining or drilling activity.*—Dumps, headframes, shafts, portals, well sites, drill holes, other excavations, graded terrain, general terrain alterations, and new activity in an abandoned mine area.
3. *Instrumentation.*—Bunkers, diagnostic buildings, observation sites, communication lines, powerlines, evidence of removed buildings or equipment, survey lines, and triangulation stations.
4. *Habitation.*—Camps (trailers or tents), camouflaged areas which had been campsites, guardhouses, fences or evidence of removed fences, security gates, trash and garbage dumps or evidence of burial of waste debris, and disrupted vegetation.

B. Blast effects

1. *Terrain alterations.*—Craters, slides, large faults and fractures, disrupted vegetation, change in reflectance of vegetation on camouflage detection film, dust clouds, radioactivity, melted snow, and disrupted streamflow.

II. Ground inspection

A. Site preparation

1. *Vehicle.*—Oil drops, tire tracks (particularly heavy or abnormal vehicles), parts, fuel dumps, fuel spillage, small roads, special parking areas, dust on leaves, vehicle parts, and killed vegetation.
2. *Mining activity.*—Portal (tunnel or shaft), dumps (look for valuable minerals) drill holes (watch for large drill holes), drill cuttings, scrapes on rocks from equipment, evidence of buildings, cable marks, hard hats, picks, drills, crowbars, and unusual neatness of old mining area.
3. *Instrumentation.*—Polyethylene insulation, shield braid, jacket from coaxial cable, polyvinyl tape, small diameter solder, small soldering irons, metallic shell connectors, desiccant containers, plastic film envelopes or wrapping, portable electric power sources (storage batteries, primary cells, engine-driven generators, transducers and mountings for them), shock mounts, magnetic tape, photographic paper, film, film containers, film spools, fragmentary test calculations on walls, abandoned electrical scrap of any kind or containers, vacuum tubes, transistors, wire, cable, instrument holes in strategic locations, screwdrivers, solder tips, sandbags, lead bricks, and concrete.
4. *Habitation.*—Excessive habitation or better than that of normal mining area, trailer camp or tent camp, garbage and trash dumps, cigars, cigarettes, pipes, matches, tobacco cans, bottles, sanitation refuse, abandoned clothing, shoes, boots, security fencing, special recreational facilities, fencing, paper products, office equipment, drafting equipment, letterheads, and technical magazines.

B. Blast effects

1. *Terrain alterations.*—Fractures in ground, rocks, bedrock, disturbed humus, flipped boulders, pebbles in bushes, depressions over collapsed explosion cavities, spalling of rocks, faults, small rockslides and rockfalls, boulders rolled down the hill, road and fence displacement, stream disruption, vegetative disruption (fallen dead trees), mudflows, and snow avalanches.

III. UNDERGROUND EXAMINATION

A. Site Preparation

A. Site preparation

1. *In special test mine.*—(Instrumentation clues listed earlier are as applicable underground as on the surface.) Cable supports and/or evidence of removal, bulkheads, blast door, extra bracing and unusually strong timbers, numerous survey markers and marks, unusual mining plan, no evidence of commercial products, no mined stopes, electronic waste, sandbags, and unusual number of lights or light outlets.

2. *In ore-producing mine.*—Look for new work in an abandoned part of the mine, or new workings out of the ore zone. Anomalous activity in mine.

B. Blast effects

Fractures, faults (orientation, opening, and displacement of old fractures), spalling, shape of drift, broken sets, closed tunnels, alinement and grade of tunnel destroyed, altered tunnel shape, broken and twisted air or water lines, rails, cables, offsets, and cut and fill of tunnel floor to establish grade.

Appendix No. 2. Discussion on Inspection Techniques

AERIAL TECHNIQUES

The first technique to be used by an inspection group in most cases would be aerial observation (in conjunction with aerial photography) at elevations from 500 to 5,000 meters. In the case of major earthquakes or poorly concealed nuclear explosions, the ability to positively identify a seismic event by aerial observation is probably excellent. However, it is unlikely that simple aerial observation will suffice to identify most seismic events detected by the teleseismic net. An earthquake that will produce a seismic signal of the magnitude expected from a nuclear experiment is not likely to cause any significant surface disruption and it is assumed that most nuclear explosions would be contained. In addition, the need for favorable weather and light conditions as well as the limited human ability to recognize and record the many details observed in a short period of time are serious limitations on the use of aerial observation as an inspection method. Despite these disadvantages and limitations aerial observation would always remain an important technique in onsite inspections because of the obvious ease with which it can be employed. It is widely applicable and requires no special equipment or devices beyond a suitable aircraft.

Aerial photography has some of the same limitations and disadvantages of aerial observation in onsite inspection systems. It possesses, however, the unique ability to record information with high density and affords the observer a precise record that can be studied for details missed by the human observer. Aerial photography can extend the field of human vision by selecting and intensifying certain diagnostic reflectance features of the area to be investigated. For example, infrared sensitive emulsions can detect changes in vegetation not discernible by the human eye. It is anticipated that aerial photography would be used in all onsite inspections for study and interpretation of human activity, and of geological and terrain features. Aerial photographs at various scales and different film-filter combinations used in Operation Hardtack II and in various earthquake investigations in the world reveal that aerial photography can identify and differentiate between unconcealed nuclear experiments and major earthquakes.

Aerial radiation surveys which are based on measuring the gamma radiation from surface sources can be useful for onsite inspection only if the nuclear detonation vents to the atmosphere. While this is not likely in a nuclear experiment in which the design calls for complete containment, the possibility may exist. It is obvious, of course, that aerial radiation will have no value in identifying a seismic event as an earthquake. Despite the limitations inherent in aerial radiation surveys, the effort required to conduct the work is nominal. For this reason, it is expected that survey aircraft used for onsite inspection would be equipped with radiation-measuring instruments.

The measurement of the geomagnetic field from the air by either fixed-wing or helicopter aircraft would be useful for the detection of ferromagnetic objects connected with human activity at detonation sites. Like most aerial surveys employing geophysical techniques, aeromagnetics is ineffective in identifying or

discriminating between natural earth shocks and nuclear detonations. It is not expected, for example, that a natural earth shock would produce measurable magnetic field variations.

Aerial electromagnetic surveys have the same limitations of use for onsite inspection as aeromagnetic surveys. The basic difference is the ability of aerial electromagnetic techniques to detect nonmagnetic conductors. It is expected that aerial electromagnetic surveys and aeromagnetic surveys would be made simultaneously. Inasmuch as the capability of detection is only a few hundred feet and is always a function of the greatest dimension of the conductor, these surveys will almost certainly need to be conducted from helicopters.

Infrared detectors can be used to measure temperature variations on the earth's surface, such as those caused by surface soil disturbance or by heat sources associated with human activities at the site. This method differs from infrared photographic surveys since it depends upon the measurement of heat sources and not on reflectance properties of the ground surface or vegetation. This method may detect heat from small sources despite their concealment. However, heat generated by a nuclear detonation at containment depth is not likely to be detectable. This survey technique has an unknown potential in onsite inspection.

SURFACE TECHNIQUES

If aerial observation, aerial photography, and the instrumental aerial surveys fail to identify the cause of the seismic event, it would be necessary then to carry out visual and instrumental examinations on the surface. Surface inspection (on foot, horse, or appropriate vehicle) of all features associated with human activity and with the geology and terrain is the first step in the detailed phase of the inspection system. Deep soils, heavy vegetation, rugged topography, snow, and ice would limit the examination. Terrain alterations can be prevented also by deep burial of the experiment. Despite these limitations, visual examination will provide effective means of distinguishing a natural earth shock from a nuclear event. It is expected that most earthquakes, except those of very small magnitude, will produce some evidence of its occurrence to the trained observer. Visual observations of human activity and the features caused by that activity may be very helpful. Note the attached appendix of clues. However, visual observations cannot, except under the most favorable circumstances, be expected to confirm or determine the exact site of a nuclear explosion. For greater precision, other more sophisticated and complex ground survey techniques must be employed.

The vertical motion of the earth's surface after nuclear explosions, and in some cases after natural earth shocks, affords a potential method of identifying seismic events. Such measurements are relatively easy to accomplish by liquid level, electronic measuring, or optical methods. For example, on Operation Hardtack II appreciable and detectable changes in vertical motion were recorded. It is a valuable technique for limited areas but rough topography will restrict its application.

The measurement of horizontal displacement of preexisting fixed points with high order of accuracy is used in major earthquake investigations. It would have a limited use as an intermediate inspection technique for the identification of seismic events if the necessary control points existed in the area under inspection. In general, it is expected that measurements of horizontal motion will only detect or confirm the occurrence of a major earthquake in the area of interest. For this reason it cannot be considered as useful as vertical motion surveys.

Ground radiation surveys, like aerial radiation surveys, will only be rewarding as an inspection technique if the detonation vents to the atmosphere. It would not be effective for earthquake identification.

Both ground magnetic and electrical surveys have limited usefulness in the detection of artifacts associated with nuclear detonations. These methods can be used either to locate more precisely ferromagnetic or conducting materials previously located by aerial surveys or as a means in themselves to comb a suspected area for evidence of human activity. The depths of penetration are usually better when used on the ground than in the air for the same techniques.

The use of seismic refraction and reflection techniques, used extensively in petroleum exploration to locate anomalous subsurface features, might be used successfully to detect cavities produced by nuclear explosions. Although the technique may be hindered by unfavorable geologic conditions, it appears to have potential as an onsite inspection technique and warrants further evaluation.

The detection of seismic signals emanating from disturbed subsurface conditions caused by a seismic event is a method of potential value in onsite inspection. In the Blanca event at Hardtack II the recorded aftershocks were similar to those produced by earthquakes. Although this method requires a fair amount of equipment and of time for observations, its value as an onsite inspection method is considered to have some promise. The presence of natural microseisms may mask those seisms produced by nuclear detonations and a conducted research program on aftershocks from artificial and natural seismic events is necessary to determine the magnitude and extent of seismic noise from all sources.

The measurement of trace elements in soil may be used to detect the presence of masses having different chemical composition from the enclosed media. It is commonly used in exploration for metallic ore bodies to detect small amounts of metal diffused through the country rock from the concealed ore body. The application of this technique to inspection would only be useful if venting took place. However, even then the results would be in doubt since the diffusion of radioactive gases from deeply emplaced nuclear detonations under nonventing conditions is slow.

The analysis of plant life at Hardtack II on Rainier Mesa did not disclose any biological absorption of radioactive fission products. Since plant absorption is slow and depends upon the porosity and permeability of the subsurface to ground water, the technique does not appear to afford any promise as an inspection method except in unusual circumstances. Similarly, geochemical analyses of ground water cannot be considered to have a high potential value for use in an inspection system, except where there may be conditions of rapid movement of water.

DRILLING

The third and final phase of onsite inspection—drilling—includes many standard and well known methods used in mineral exploration and petroleum production. They involve petrological and mineralogical analyses of drill cores and cuttings, radiation and neutron logs, many electrical logging techniques, geothermal surveys, sonic logs, and photographic logs. But since the objective of the drilling phase is to collect a sample of radioactive fission products as positive evidence of nuclear detonations all these techniques serve only to expedite and control the drilling process. The objective can be achieved only by successful completion of the first two phases of inspection. Unless these two phases result in selecting a reasonably small target area, preferably less than 500 feet radius, little success can be expected from the drilling phase.

Drilling will be the most expensive and logistically difficult of the three phases Indiscriminate drilling with the hope of intersecting a target appears to be out of the question because of the time and expense involved. It has been estimated that to insure the discovery of a cavity or shattered zone produced by a 20-kiloton explosion within a circular area of about 3 square miles, more than 250 holes would have to be drilled. If all 250 holes were drilled to scaled depth, it would involve about 275,000 feet of drilling. The estimated cost would be $1,500,000 under the best conditions and would take 2 years to complete.

UNDERGROUND AND MARINE

Techniques that might be used for inspection purposes underground and in the marine environment differ only in detail from techniques used in land-surface inspection. Underground, a controlling if factor is the limited physical access to a smaller area of operation. In the marine environment, the water surface, on which operations take place, is featureless.

In the underground environment human activities would be more closely confined and geologic features easier to recognize because of better exposures. The confined nature of activities limits the area to be examined but would also permit easier concealment of clandestine operations. In an operating mine, for example, nuclear experiment activities may be difficult to distinguish from normal and legitimate mining operations.

In marine areas the fluid nature of the environment would almost immediately erase surface manifestations of a seismic disturbance whether it were an earthquake or nuclear detonation. However, visual and photographic analysis of the water mass might be helpful to detect the presence of suspended sediments. Although disruptions of the bottom caused by a seismic event would tend to be smoothed out and destroyed, the use of underwater TV and sonic techniques, such as sonoprobe, might detect such disturbances. The easiest items to detect would be the artifacts. Items such as drill casing and undersea cable necessary to conducting a test may be detectable with magnetic and electrical surveys. Evaluation of these techniques in the marine environment is dependent on actual use.

APPENDIX NO. 3. SUMMARY OF SUGGESTED RESEARCH ON ONSITE INSPECTION TECHNIQUES

1. Current assessment of the state-of-the-art and direct application in future tests under various environmental conditions for the improvement of infrared, magnetic, electrical, electromagnetic, radiation, seismic, and vertical displacement surveys, and of geochemical analyses of radioactive trace elements in water.
2. Studies of the aftershocks of explosions and earthquakes and establishment of their relationship to each other and to different worldwide seismic environments.
3. Development of aerial techniques involving nonphotographic sensors for the detection of human activity.
4. Botanical studies to relate vegetation damage to shocks from explosive and earthquake sources and development of the means by which such damage may be detected at an early stage.
5. Development of new film and photographic techniques for measurement and evaluation of explosive and earthquake effects and definition of environmental conditions pertinent to the limitation of photographic capabilities.
6. Marine biological studies to establish sensitivity of life forms to explosive and earthquake shocks.

Representative HOLIFIELD. Our next witness will be Dr. Roland Beers, consultant to the petroleum industry in seismic techniques. I would like to add that he has had a long and distinguished career as a consultant to the oil industry, and has recently been active in prospecting by scientific means for metal deposits.

I might also say that I have it from a normally reliable source that Dr. Beers is in actuality the old prospector of whom we heard the legend in the panel discussion this morning.

The committee wishes to express its appreciation to you for your appearance today. It is always a privilege to call on representatives from industry, who, from experience in the field, are able to contribute to our consideration of the technical problems in these hearings.

Please proceed.

STATEMENT OF DR. ROLAND BEERS,* CONSULTANT, SEISMIC TECHNIQUES

Dr. BEERS. Thank you, Mr. Chairman and members of the committee. May I thank you for the opportunity to present these views.

Representative HOLIFIELD. I might say before you start that it might be necessary for us to adjourn in case of a rollcall. The mutual security bill is on the floor this afternoon, and we might have to answer the bells.

Dr. BEERS. I will try to be as brief as possible.

Detection of cavities and fractured zones resulting from underground nuclear explosions by seismic profiling: Exploration for petroleum and minerals has much in common today with detecting the

*Geophysics, Owego, N.Y., June 6, 99; m. 21; c. 2E.E., Rensselaer Polytech, 21; M.S., Mass. Inst. Tech, 28, Ph. D. (geol), 43; Harvard, 40–41. Instr. physics and elec. eng. Rensselaer Polytech, 21–22; develop. engineer, West. Elec. Co, N.Y., 22–23; develop engineer, Raytheon Mfg. Co, Mass, 25–26; physicist, Submarine Sig. Corps, Boston, 27–28; party chief, Geophys. Research Corp, Houston, 28–31; Geophys. Service, Inc, Dallas, 31–34, v. pres. 34–36; pres. and director Geotech. Corp, Texas, 36–52, chairman board directors, 37–52; Lecturer, Rensselaer Polytech, 47–48; Prof. Geophys, 48–; Consultant and contractor, U.S. Govt, 47–Research assoc, Mass. Inst. Tech. 43–46; pres. and director, Geotech. Corp. Ltd, Montreal, Can. 44–52; consulting geophysicist, U.S. Geol. Surv. Wash., D.C. 43–46; chairman board Geotech. Corp. of Del, Dallas, 46; partner, Beers and Heroy, Dallas, 46–; expert consultant, Joint Research & Develop. Board, Wash. D.C., 46. With Atomic Energy Com; USN, 44, USA, 17–19, AA; Soc. Explor. Geophys; Asn. Petrol. Geol; Accoustical Soc; Inst. Min. & Metal. Eng; Inst. Radio Eng; Seismol. Soc.; fel. Geol. Soc; fel. Geog. Soc; Ceramic Soc; Soc. Econ. Geol; Soc. Explor. Geophys; Geophys Union; Photog. Socl; Fel. Am. Acad; Fel. N.Y. Acad Radioactivity in rocks and sediments; seismic investigations of earth's crust; sedimentary rocks; exploration for petroleum; geological aspects of electromagnetic wave propagation on the ground; determining physical properties of rock strata.

site of underground nuclear explosions. In a general way, we know where to begin our search, for the area has been narrowed down to a few tens of square miles by previous work. The task before us is to pinpoint the site with sufficient precision to justifying drilling one or more holes.

Dr. Foose has reviewed other geophysical methods used in exploration for oil and minerals. Some of these are performed in the air, resulting in a rapid outline of one or more prospective targets. These were selected from an area of a few hundred square miles. The precision of measurements is not sufficient to commence drilling operations, and some intermediate step is required to reduce the amount of drilling.

The electrical and magnetic methods used in the aircraft may also be employed on the ground. Their precision is greater on the ground provided there are specific objects which can be detected by these methods. But if the designer and operator of the clandestine explosion have been thorough and careful, they have removed all such evidence, and we are left with only traces of his activities and the hole in the ground. It is my purpose to describe for you one method which seems worthy of further investigation, to locate this hole—or the fractured ground around it.

This method uses the exploration seismograph, similar in principle to the earthquake seismograph. The major difference is that these instruments are adapted to record the seismic waves generated by a small explosion of dynamite in a shallow, tamped drill hole. As a result of the explosion, seismic waves radiate in all directions into the earth, where they are reflected and refracted. Some of them return to the surface. By observing close details of the returning waves, valuable information may be gained on the structure, attitude, and condition of rocks beneath.

This simple principle has been used for more than 30 years with considerable success. It is now considered an indispensable tool of the exploration industry. In recent years its use has been extended in the search for minerals, water resources, and to engineering problems such as highway and foundation design and construction. It has proved to be very practical. Success in its use depends upon the employment of experienced personnel in both application and interpretation. Its full scope and limitation in detecting explosion sites can readily be determined by trial on a few specific assignments.

I should now like to show you an example of the manner in which this method may be used for detection purposes. This use of the method is new. It has resulted from the examination of a very large number of seismic records during the past 6 months with this specific application in mind. While this particular application is new, many competent seismologists have employed the principles used, and they will recognize the use of old and familiar techniques to solve a new problem.

Figure 1 shows schematically the arrangement of seismograph instruments employed in petroleum exploration. This is a cross section of the earth, showing the line of seismometers spread out in a horizontal line along the surface. In the center of the line and at each end you will note a shothole. A charge of dynamite is lowered into the central hole. An electric blasting cap will detonate the charge. Wires leading up the hole connect with the recording device and, at

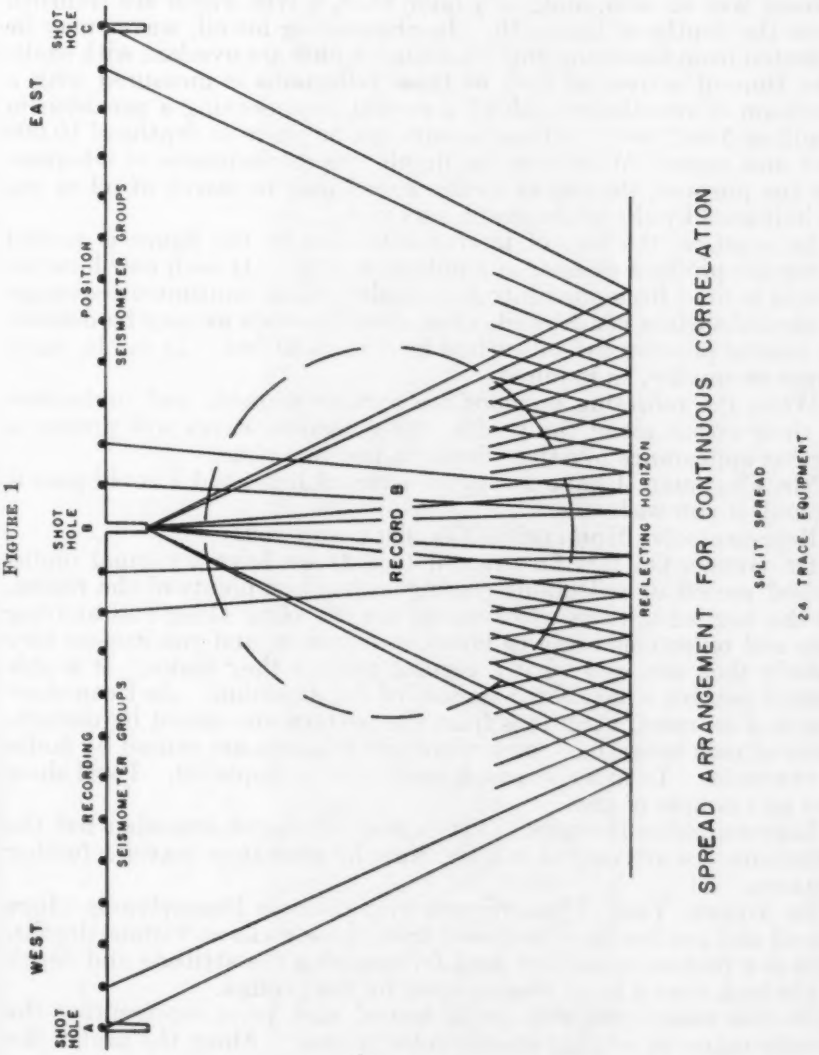


FIGURE 1

SPREAD ARRANGEMENT FOR CONTINUOUS CORRELATION

a signal from the observer, the charge is fired. The hole is well tamped with mud and water, so that the charge does not blow out from the hole. The well-tamped charge produces the clearest waves, and every effort is made to insure complete tamping.

Just before the shot is fired, the observer starts a recorder in operation. On the record the waves traveling along the surface of the ground will be seen, and, at a later time, waves which are returned from the depths of the earth. In prospecting for oil, waves may be reflected from limestone and sandstone, which are overlain with shale. The time of arrival of each of these reflections is measured with a precision of one-thousandth of a second, representing a precision in depth of 5 to 7 feet. Measurements can be made to depths of 10,000 feet and more. Whatever the depth, the performance is adequate for the purpose, that is, to locate a drill hole in search of oil or gas as indicated by the seismograph survey.

In practice, the line of instruments seen in the figure is moved along the profile a quarter of a mile at a time. At each new location a shot is fired from the central shothole. Thus continuous coverage of the subsurface is achieved, at as close intervals as may be desired. In general practice the subsurface interval is 50 feet. It can be made larger or smaller, as required.

When the reflecting horizons are regular, smooth, and continuous in their extent along the profile, the reflection waves will present a regular appearance like that shown in the next slide.

Mr. Chairman, I have a copy of a record here, and I could pass it around, if you wish.

Representative HOLIFIELD. Yes, sir; thank you.

Dr. BEERS. On this first record (fig. 2) we have a normal undisturbed record of reflections coming at various points of the record. At the beginning the surface waves are traveling along and arriving here and reflections begin to arrive in sequence, and you can see how orderly they are, and what a regular pattern they make. It is this regular pattern which is the essence of the detection. As I can show you in a moment, variations from the pattern are caused by disturbances of rock beneath. Some of the disturbances are caused by faults in the rocks. In other words, a rock may be displaced. I will show you an example of that.

Representative HOSMER. That is from the same explosion but the reflections are arriving at a later time, because they travel a further distance.

Dr. BEERS. Yes. These records were taken in Pennsylvania where the oil and gas has been produced from these rocks at various depths. This is a routine procedure used for mapping the attitude and depth of the rock over a large area covered by the profiles.

On this record you will see 24 traces, each trace representing the records taken at a single seismometer station. Along the profile the stations are 100 feet apart, giving the 50-foot interval subsurface shown in the previous diagram.

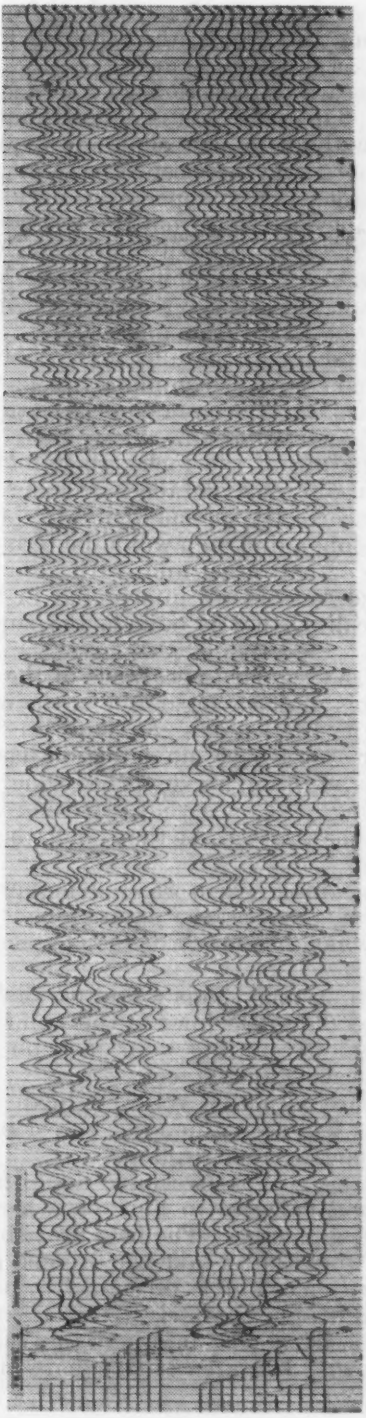


FIGURE 2

When the rocks at depth are fractured, or faulted, so that there is a discontinuity along the profile, the reflected waves are irregular in the fractured zone, as shown in figure 3.

This again is a record of reflections of rock taken at approximately the same locality. This reflection is one without interruption, this is another reflection without interruption, and here we notice a distinct break. The characteristic between this and the location of the cavity that this discontinuity appears on only one horizon (fig. 4). Here we see reflections arriving from the upthrown side of the fault and there you see the downthrown fault, from which the reflections arrive at a later time. The mapping of faults in this manner is one of the important functions of the seismograph in petroleum exploration. Once the structure has been mapped in this way along a profile, other profiles are laid out in parallel and other directions, so as to cover the exploration target as completely as possible.

When all the records have been taken, the seismologist and geologist begin their task of interpretation. First, there must be a thorough knowledge of the nature of the rocks, their folding and faulting on the prospect. Without this detailed geologic knowledge, the recordings may be incorrectly interpreted. In locating the site of an underground explosion, a first step would be to take a few preliminary records outside the actual target and, with knowledge of the geology of the prospect, the records are interpreted in terms of the normal behavior in the locality, that is outside the zone, disturbed by the explosion.

You have noted the regular appearance of the seismic records over a normal area. This uniform regularity is the essence of our ability to accomplish cavity detection. As long as the reflected waves present this regular appearance, no detection has been accomplished. When the waves travel through a disturbed zone, the contrast in their appearance is striking. At once your attention is drawn to the jumbled appearance of the traces. At that point the search settles down in earnest.

I will show you a sample which was taken over a mined-out area in Pennsylvania. In some cases the cavities had been refilled, so we penetrated a zone which has many of the characteristics of an underground explosion.

Representative HOLIFIELD. This would parallel a cavity where——

Dr. BEERS. You could settle down at this point, having located this, and get closer detail by putting your instruments closer together. This cavity width is actually 400 feet.

Representative HOLIFIELD. If you were in an area where a number of these natural, or let us say non-nuclear, conditions existed, this would further complicate your drilling.

DR. BEERS. Yes, sir. What we have done in Pennsylvania is to map the underground galleries to find out where they are in order to avoid this kind of difficulty. The field procedure in carrying out a search of this kind would be to take the original area which has been assigned in which an epicenter is presumed to occur. I have taken a sample here which is approximately 10 square miles. By the other geophysical methods, by all the information available, we have narrowed the search down to an area of 10 square miles.

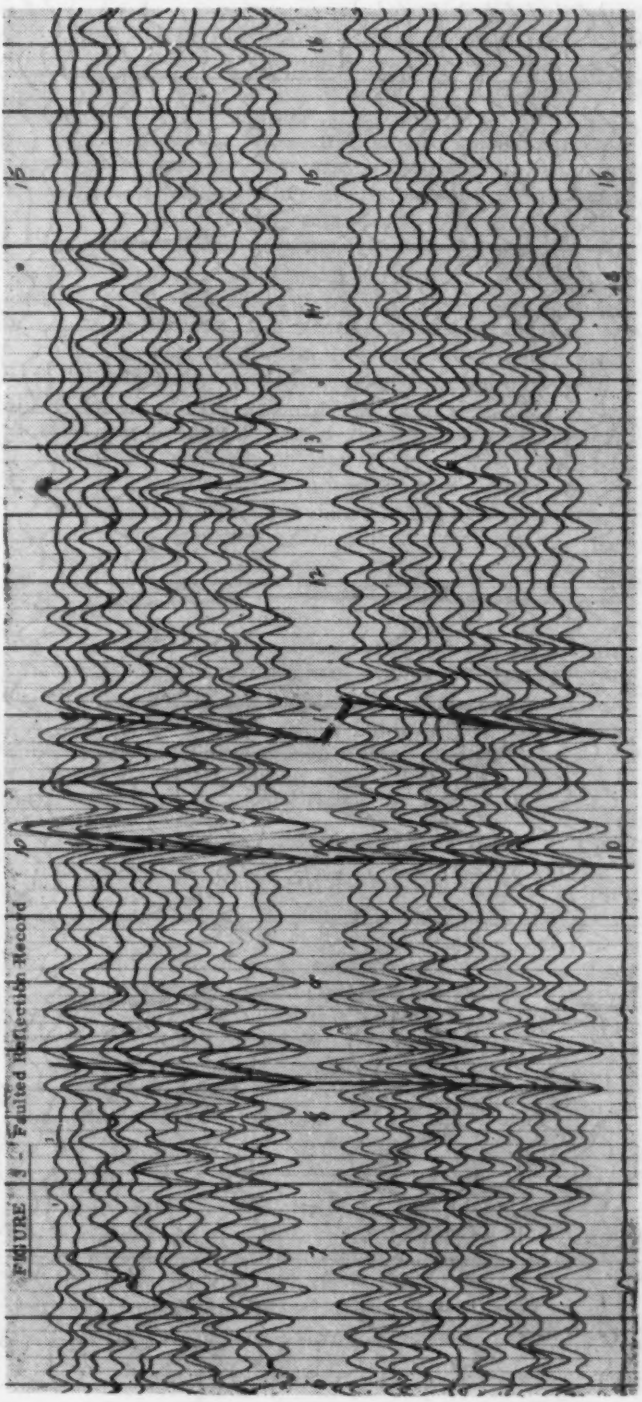


FIGURE 3

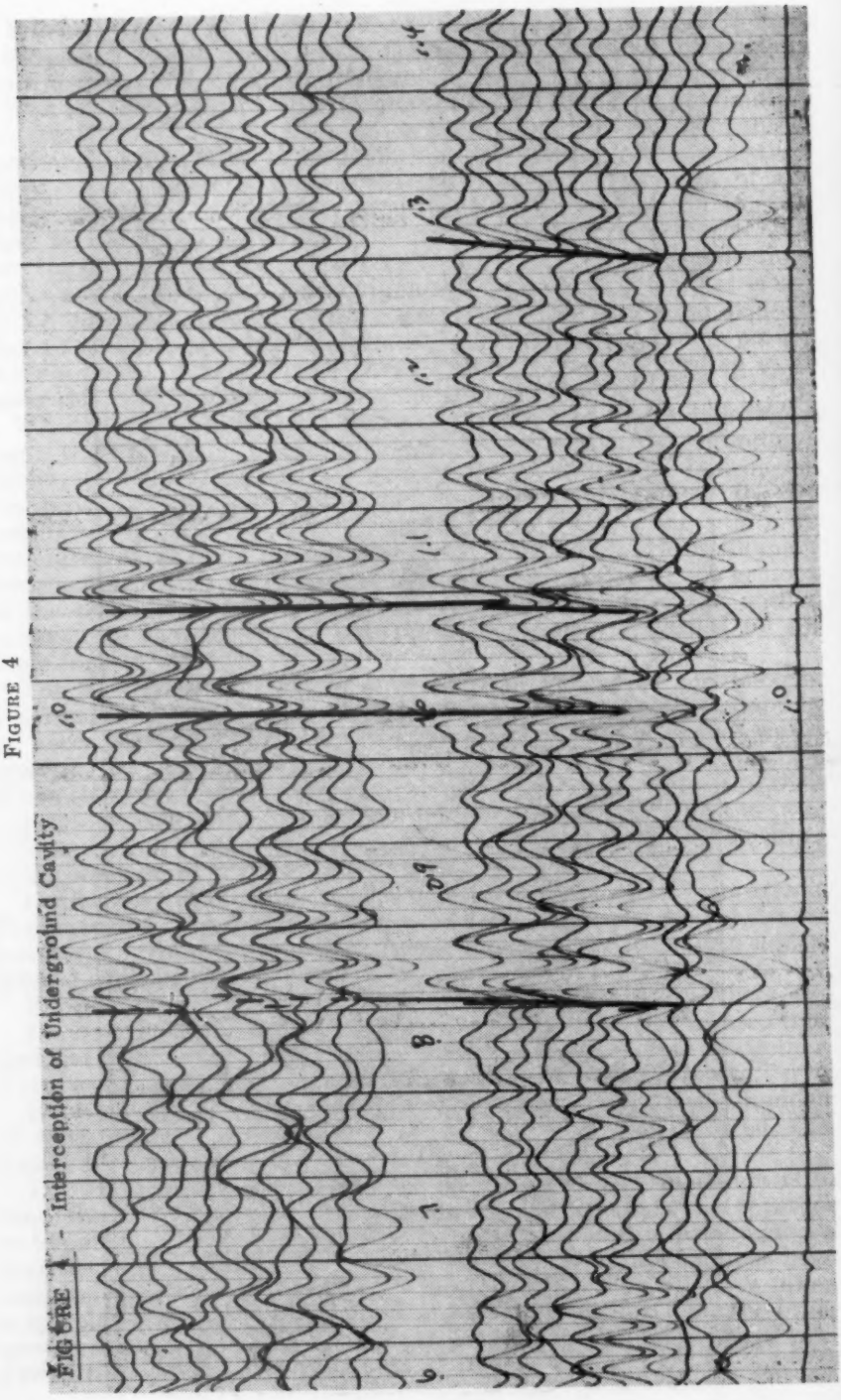


FIGURE 4

We are assuming a target which has a radius for this discussion of 500 feet. This is a fairly large radius. I will use it simply for an example. The target area we are investigating is a little over 3 miles on a side, and we will cover this with profiles spaced 400 feet apart. This will require 500-shot points and profiles.

On all those profiles not intersecting the fractured zone, the reflections will show the regular appearance shown to you previously. Where the profiles intercept the disturbed zone, the waves show irregularities in times of arrival, phase relations among the various traces, erratic changes in wave amplitudes. In general, there is a condition of disorder in contrast with the normal appearance of uniform regularity. The observation of this disturbed condition constitutes the act of detection. If it is desired to pinpoint the cavity zone more precisely, the seismometers can be placed more closely together for a few extra shots. Thus, the precision can be of the order of a few tens of feet.

I have shown you a few sample records of the reflection type. This method is recommended for the initial trials over sedimentary formations, where the layered sequence of beds will produce several reflections. Over a salt dome, it is possible that the refraction method would be more effective. Usually it has been difficult to interpret the reflections around salt domes. As suggested earlier, some research should be done on both methods in order to devise the best techniques for cavity detection under several conditions.

Some rough estimates have been made of the time and cost to conduct surveys of this type. The best figures we have are those of current commercial work in the United States. The records on which this analysis is based were taken in the rugged terrain of western Pennsylvania. Much of the work was done in severe winter weather. Not all such work is as difficult as this. The example is being used, since the records were taken under these conditions and thus represent actual field experience.

On this work the cost of each profile has averaged about $250. A single field party composed of about 10 men, 2 drills, equipment, and 5 trucks, has completed about 200 profiles per month. Over the water in the Gulf of Mexico, a party, with 2 vessels, has completed as many as 1,000 profiles per month. In the example chosen for cavity detection, 500 profiles would cost a maximum of $125,000 over land, and require about 2.5 party-months. Over water, such a survey could be completed in less than 1 party-month, with an estimated total cost of about $60,000. (These estimates of cost and man-hours may be modified after trial of the method in the field. Some improvement in the performance is expected.) Should detection be accomplished prior to the completion of the entire survey, the remainder could be abandoned.

If detection is not accomplished upon completion of a grid survey, it would be necessary to explain the anomaly which first brought attention to the prospect. All previously assembled data would be reviewed, including the seismic profiling, and the search should not be abandoned until the anomaly has been satisfactorily accounted for. It is probable that more than one target area would have to be investigated. The same procedure would apply to each target in turn until detection has been accomplished, and proved by the drill.

Representative HOLIFIELD. Thank you, sir.

Are there any questions of Dr. Beers?

Representative HOSMER. Can you use this method, Dr. Beers, where the surface itself is highly irregular?

Dr. BEERS. Mr. Hosmer, in Pennsylvania the mountains are the result of dissection of the uplifted plateau and under extreme conditions we do work up and down the valley slopes. There are places that are almost impossible to get to. I should say this is a matter of the incentive, of the dollars and cents you have to spend for the detection.

Representative HOSMER. I believe in your statement you reviewed the fact that you have to get out away, or at least on the side of a suspected area and get yourself some geology.

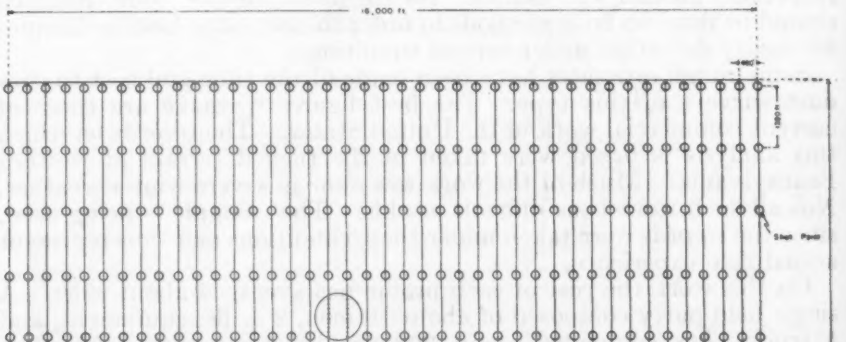
Dr. BEERS. A little; yes.

Representative HOSMER. Against which you can later compare your geology in the suspected area; is that right?

Dr. BEERS. Yes, sir. If you will look at the diagram before you there (fig. 5), you will see that the area we plan to explore is so much greater than the target area, that this should not be a major problem.

Representative HOSMER. I am wondering what degree of success do you have in really nailing down the subsurface.

FIGURE 5



Schematic Diagram Showing Arrays of Seismograph Profiles

Dr. BEERS. In petroleum exploration, for example, at the present time, using this method, the reflections which are mapped, are within an error of plus or minus 30 feet, as regards depth.

Representative HOSMER. The company does not guarantee any dry holes?

Dr. BEERS. The presence of petroleum is inferred from certain structural and other conditions.

Representative HOSMER. Incidentally, at what depth does this produce meaningful indications?

Dr. BEERS. The first record I presented shows reflections to greater than 20,000 feet.

Representative HOSMER. That covers all the ranges we would be interested in.

Dr. BEERS. In most instances it covers the full range accessible by the drill, which is about 25,000 feet.

Representative HOSMER. Thank you.

Representative HOLIFIELD. Thank you very much, sir.

(The full statement of Dr. Beers follows:)

DETECTION OF CAVITIES AND FRACTURED ZONES RESULTING FROM UNDERGROUND NUCLEAR EXPLOSIONS BY SEISMIC PROFILING

Exploration for petroleum and minerals has much in common today with detecting the site of underground nuclear explosions. In a general way, we know where to begin our search, for the area has been narrowed down to a few tens of square miles by previous work. The task before us is to pinpoint the site with sufficient precision to justify drilling one or more holes.

Dr. Foose has reviewed other geophysical methods used in exploration for oil and minerals. Some of these are performed in the air, resulting in a rapid outline of one or more prospective targets. These were selected from an area of a few hundred square miles. The precision of measurements is not sufficient to commence drilling operations, and some intermediate step is required to reduce the amount of drilling.

The electrical and magnetic methods used in the aircraft may also be employed on the ground. Their precision is greater on the ground, provided there are specific objects which can be detected by these methods. But if the designer and operator of the clandestine explosion have been thorough and careful, they have removed all such evidence, and we are left with only traces of his activities and the hole in the ground. It is my purpose to describe for you one method which seems worthy of further investigation, to locate this hole—or the fractured ground around it.

This method uses the exploration seismograph, similar in principle to the earthquake seismograph. The major difference is that these instruments are adapted to record the seismic waves generated by a small explosion in a shallow, tamped drill hole. As a result of the explosion, seismic waves radiate in all directions into the earth, where they are reflected and refracted. Some of them return to the surface. By observing close details of the returning waves, valuable information may be gained on the structure, attitude, and conditions of rocks beneath.

This simple principle has been used for more than 30 years with considerable success. It is now considered an indispensable tool of the exploration industry. In recent years its use has been extended in the search for minerals, water resources, and to engineering problems such as highway and foundation design and construction. It has proved to be very practical. Success in its use depends upon the employment of experienced personnel in both application and interpretation. Its full scope and limitation in detecting explosion sites can readily be determined by trial on a few specific assignments.

I should now like to show you an example of the manner in which this method may be used for detection purposes. This use of the method is new. It has resulted from the examination of a very large number of seismic records during the past 6 months with this specific application in mind. While this particular application is new, many competent seismologists have employed the principles used, and they will recognize the use of old and familiar techniques to solve a new problem.

The first slide (fig. 1, p. 307) shows schematically the arrangement of seismograph instruments employed in petroleum exploration. This is a cross section of the earth, showing the line of seismometers spread out in a horizontal line along the surface. In the center of the line and at each end you will note a shot hole. A charge of dynamite is lowered into the central hole. An electric blasting cap will detonate the charge. Wires leading up the hole connect with the recording device, and, at a signal from the observer, the charge is fired. The hole is well tamped with mud and water, so that the charge does not blow out from the hole. The well-tamped charge produces the clearest waves, and every effort is made to insure complete tamping.

Just before the shot is fired, the observer starts a recorder in operation. On the record the waves traveling along the surface of the ground will be seen, and, at a later time, waves which are returned from the depths of the earth. In prospecting for oil, waves may be reflected from limestone and sandstone, which are overlain with shale. The time of arrival of each of these reflections is measured with a precision of one-thousandth of a second, representing a precision in depth of 5 to 7 feet. Measurements can be made to depths of 10,000 feet and more. Whatever the depth, the performance is adequate for the purpose, that is, to locate a drill hole in search of oil or gas as indicated by the seismograph survey.

In practice, the line of instruments seen in the figure is moved along the profile a quarter of a mile at a time. At each new location a shot is fired from the central shot hole. Thus continuous coverage of the subsurface is achieved, at as close intervals as may be desired. In general practice the subsurface interval is 50 feet. It can be made larger or smaller, as required.

When the reflecting horizons are regular, smooth and continuous in their extent along the profile, the reflection waves will present a regular appearance, like that shown in the next slide (fig. 2, p. 309). On this record you will see 24 traces, each trace representing the records taken at a single seismometer station. Along the profile the stations are 100 feet apart giving the 50-foot interval subsurface shown in the previous diagram.

When the rocks at depth are fractured, or faulted, so that there is a discontinuity along the profile, the reflected waves are irregular in the fractured zone, as shown in the next slide (fig. 3, p. 311). Here we see reflections arriving from the upthrown side of the fault and there you see the downthrown fault, from which the reflections arrive at a later time. The mapping of faults in this manner is one of the important functions of the seismograph in petroleum exploration. Once the structure has been mapped in this way along a profile, other profiles are laid out in parallel and other directions, so as to cover the exploration target as completely as possible.

When all the records have been taken, the seismologist and geologist begin their task of interpretation. First, there must be a thorough knowledge of the nature of the rocks, their folding and faulting on the prospect. Without this detailed knowledge, the recordings may be incorrectly interpreted. In locating the site of an underground explosion, a first step would be to take a few preliminary records outside the actual target, and, with knowledge of the geology of the prospect, the records are interpreted in terms of the normal behavior in the locality, that is, outside the zone disturbed by the explosion.

You have noted the regular appearance of the seismic records over a normal area. This uniform regularity is the essence of our ability to accomplish cavity detection. As long as the reflected waves present their regular appearance, no detection has been accomplished. When the waves travel through a distrurbed zone, the contrast in their appearance is striking (fig. 4, p. 312). At once your attention is drawn to the jumbled appearance of the traces. At that point the search settles down in earnest.

In order to illustrate the field procedure, let us look at fig. 5 (p. 314). Here you see a series of profiles laid out in parallel lines to cover the entire target area. If we assume the search has narrowed down to an area of 10 square miles, a little over 3 miles on a side, we will cover this area with about 500 shot points and profiles. On all those profiles not intersecting the fractured zone, the reflections will show the regular appearance shown to you previously. Where the profiles

intercept the disturbed zone, the waves show irregularities in times of arrival, phase relations among the various traces, erratic changes in wave amplitudes. In general, there is a condition of disorder in contrast with the normal appearance of uniform regularity. The observation of this disturbed condition constitutes the act of detection. If it is desired to pinpoint the cavity zone more precisely, the seismometers can be placed more closely together for a few extra shots. Thus the precision can be of the order of a few tens of feet.

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If detection is not accomplished upon completion of a grid survey, it would be necessary to explain the anomaly which first brought attention to the prospect. All previously assembled data would be reviewed, including the seismic profiling, and the search should not be abandoned until the anomaly has been satisfactorily accounted for. It is probable that more than one target area would have to be investigated. The same procedure would apply to each target in turn until detection has been accomplished, and proved by the drill.

Representative HOLIFIELD. Our next witness is Dr. Raymond Peterson, vice president and director of research of the United Geophysical Corp. of Pasadena, Calif. He is a geophysicist with many years of experience in the practical development of techniques. Dr. Peterson will be assisted in his testimony by Mr. Frank Coker, director of research of United Dynamics Corp., of Pasadena. These gentlemen will report the results of a study just completed on the theory and practical aspects of large arrays of seismographs proposed as part of the Geneva control system.

To the best of our knowledge this is the first authoritative investigation of this subject. The committee appreciates this opportunity to hear these results firsthand.

Gentlemen, will you please come forward?

[1] These estimates of cost and man-hours may be modified after trial of the method in the field. Some improvement in the performance is expected.

STATEMENT OF DR. RAYMOND PETERSON,[2] VICE PRESIDENT AND DIRECTOR OF RESEARCH OF THE UNITED GEOPHYSICAL CORP., OF PASADENA, CALIF., ACCOMPANIED BY MR. FRANK COKER,[3] DIRECTOR OF RESEARCH OF THE UNITED ELECTRODYNAMICS CORP., PASADENA, CALIF.

Dr. PETERSON. Mr. Chairman, may I introduce Mr. Coker for a preliminary statement.

Mr. COKER. I would like to make this preliminary statement. Some of the comments we are about to hear represent early results of the research and development program which has been recommended and alluded to several times earlier today.

The particular project was under the sponsorship of the Air Force Technical Applications Center, as part of Project VELA.

Representative HOSMER. What is that?

Mr. COKER. It is a code name for this entire seismological research program which has been discussed earlier.

With that, Dr. Peterson will proceed.

Dr. PETERSON. Mr. Chairman, members of the committee, as Mr. Coker has mentioned, we have just completed a study of about 60 days duration under rather intense conditions as to the practical aspects and feasibility of installing a network of 22 seismic arrays in the U.S.S.R. (fig. 1).

The figure of 22 was used because it had been originally suggested as a figure that might be negotiable or might be accepted. It turns out to be 22 instead of 21 because one of the stations was near Vladivostok and could be installed either in Russia or China. The main conclusion of this study I think might be summed up rather simply, although this is certainly a very brief report of the results.

The conclusion is, Mr. Chairman, that the figure of five in improvement of detection capabilities which you mentioned some time ago is surprisingly enough right in the range of our results. We concluded that improvements in the range of four to six times are possible through the use of large groups of seismometers instead of single seismometers. I should qualify that; I mean five times as an average.

FIGURE 1

[2] Born in Denver, Colo., on Mar. 12, 1909. He received a B.S. degree in geology from the California Institute of Technology in 1931 and a Ph. D. degree in geophysics in 1935. He was associated with Geophysical Engineering Corp. from 1935 to 1938, and with United Geophysical Corp. from 1938 to the present time. He is vice president for research for this firm and technical consultant to United ElectroDynamics, Inc.

Dr. Peterson's principal activities have been in the field of geophysical operations and interpretation, in the design of geophysical instruments and systems, and in the development of geophysical interpretation techniques. During 1941 and 1942 he was engaged as a project leader on NDRC underwater sound transmission studies at San Diego, Calif., and on projects for development of magnetic airborne submarine detectors at Quonset Point, R.I.

[3] Has worked for 10 years in the fields of geophysical research, development, and field operations. His direct experience includes magnetic field detection, geophysical borehole logging, deep-well seismic research, seismic data computing systems, seismic data transmission equipment, ballistic missile telemetry systems, and temperature resistant coaxial cable research.

He is an honors graduate of the University of Southern California and holds the BSEE and MSEE degrees. His honors affiliations include Phi Kappa Phi, Tau Beta Pi, and Eta Kappa Nu (past president). He is registered to practice before the U.S. Patent Office bar.

He belongs to the following technical societies:

American Geophysical Union.
Seismological Society of America.
Society of Exploration Geophysicists.
Institute of Radio Engineers.
American Ordnance Association.

He holds 7 issued or pending United States and Canadian patents.

He is the joint winner of the Best Paper Award of the Year, Geophysics, for "The Synthesis of Seismograms from Well Log Data," volume XX, No. 3, 1955, and has also published and delivered numerous other technical papers at widely dispersed geographical locations, including Europe.

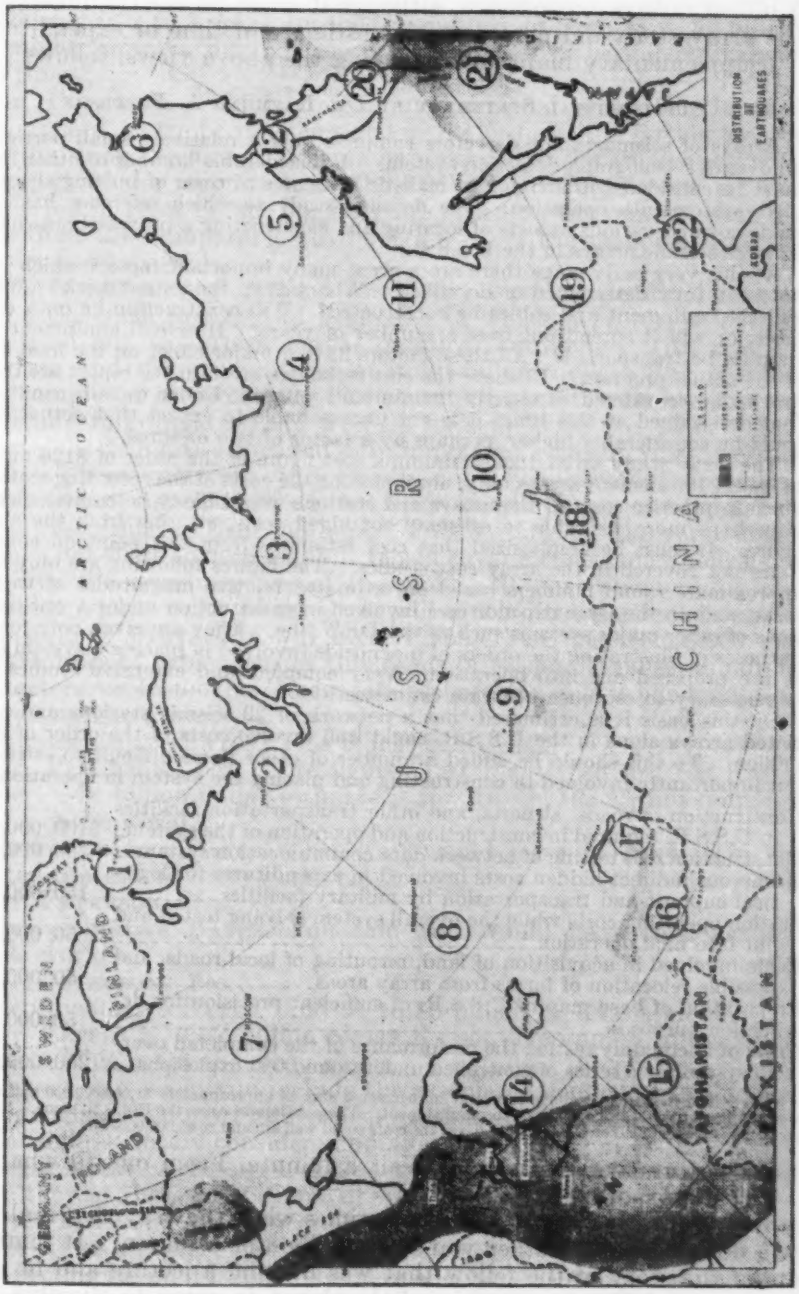

ARCTIC OCEAN
U S S R
CHINA
SWEDEN
FINLAND
ICELAND
AFGHANISTAN
DISTRIBUTION OF EARTHQUAKES

FIGURE 1

It is my personal opinion that to install a network of stations yielding this improvement will probably require 3 to 5 years, with 1 to 5 billion dollars expenditure, although it must be clearly understood the array study did not cover this estimate of time or expenditures.

(Supplementary material supporting the above views, follows:)

SUPPLEMENTAL STATEMENT BY DR. RAYMOND A. PETERSON

Arrays of seismographic detectors comprise only a relatively small portion of an overall seismograph detection system. A considerable number of other items must be considered in arriving at realistic estimates of costs of putting the complete system into operation. The detailed study to which reference has been made covered various aspects of locating and constructing a proposed network of 22 large seismic arrays in the U.S.S.R.

At this very early stage there are a great many important factors which have not been fully determined or described. Who will do the actual work? Where will the equipment and cables be constructed? Will construction be on a crash basis, or will it spread out over a number of years? How will equipment and supplies be transported? All these factors have a major effect on the final costs of the actual program. I believe the cost estimates given in the report are therefore to be considered as strictly "minimum" values. Under certain conditions (not determined at this time) it is not unreasonable to expect that actual costs could be considerably higher, perhaps by a factor of two or three.

The array study arrived at a minimum cost figure of the order of $128 million to install 100 element arrays only, not including the costs of the recording stations. Since in practice costs of the arrays and stations would likely be commingled, it is perhaps more desirable to estimate combined costs, starting from the above figure. It must be emphasized that cost estimates from this point on concern items not covered in the array cost studies. The figures following are only very approximate round numbers based on estimated relative magnitudes of various tasks, and on the large expenditures involved in construction under Arctic conditions of other major systems such as the DEW line. They are given only for the purposes of illustrating the orders of magnitude involved in placing a system such as the proposed one into operation. Very complete and extensive studies will be necessary to establish accurate estimates.

On this basis it is estimated that a network of 22 seismic stations and associated arrays alone in the U.S.S.R. could well involve costs of the order of $250 million. To this should be added a number of other items difficult to estimate, but importantly involved in constructing and placing the system in operation:

Construction of roads, airports, and other transportation facilities in U.S.S.R. required in construction and operation of the system	$100, 000, 000
Construction and testing of network data communication system	150, 000, 000
Numerous indirect hidden costs involved in expenditures for logistical support and transportation by military facilities	150, 000, 000
Station operating costs while the overall system is being tested and put into final operation	50, 000, 000
Costs involved in acquisition of land, rerouting of local roads, and possible relocation of farms from array areas	50, 000, 000
Preparation of base map of U.S.S.R. of sufficient precision for detection purposes	50, 000, 000
Costs of adequately testing the performance of the completed overall system by a series of controlled underground test explosions	200, 000, 000

NOTE.—The items listed above represent an aggregate sum of approximately $1,000,000,000 and could well require a time of 3 to 5 years for accomplishment. These estimates cover the U.S.S.R. alone. If similar systems are installed on a worldwide basis, the costs could well amount to $5,000,000,000.

Representative HOLIFIELD. Wait a minute. From one to what?

Dr. PETERSON. $1 to $5 billion.

Representative HOLIFIELD. Sometimes when they say that real fast you do not know whether you say a million or a billion. It reminds me of the story of the fellow that was making a lecture and he said in about 8 billion years this whole planet is going to dissolve. A fellow that was slightly inebriated in back of the room got up and said, "What is that you say?" The statement was repeated. This

fellow heaved a sigh of relief and said, "You had me worried. I thought you said 8 million." I wanted to be sure I got that "b" in there.

You say in 2 to 3 years, by the expenditure of from $1 to $5 billion we might improve our efficiency of detection of seismology about five times?

Dr. PETERSON. That is roughly my personal estimate. In that same regard, as we become engaged in the project, the farther we got into the thing the more I became impressed that we are dealing with a project of the magnitude of the DEW line. My figures are not supported in great detail, but they are based on the opinion that the difficulties are comparable to the DEW line, the magnitude of the project is comparable.

I have not a very detailed knowledge but I understand after a very rush crash program of 6 months in which a prototype station was tried out to furnish actual practical knowledge, with a crash program and the expenditures of multi-billion dollars, the network was completed in something like 32 months, and probably did not come into full operating effectiveness for some time thereafter. I do believe that time is an exceedingly important element when we talk about all these matters. It is strictly not a "honeymoon" operation. If we attempt to operate it on that basis we will have many "unhappy wives" returning to their mothers. This is going to take very rugged he-men out there to do this job. It will cover a continent which spans one-fifth the circumference of the globe. It covers something like 9 million square miles of area. It will involve the particular feature of installing arrays. It will involve something between 6,000 and 21,000 miles of cable, tens of thousands of telephone poles, hundreds of tractors, earth-movers, steam jets, icebreakers. It is like a military operation, like a war.

Representative BATES. This is just in Russia?

Dr. PETERSON. Yes. The figures just mentioned are for Russia alone. My billion dollar estimate is for Russia, the $5 billion applies to the entire world.

Representative BATES. Then you would have to have it in the United States and everywhere else.

Dr. PETERSON. Yes.

Representative BATES. You said you would have to cover the whole world.

Dr. PETERSON. Yes.

Representative BATES. And $1 to $5 billion in Russia alone.

Dr. PETERSON. One billion covers Russia, five applies to the whole world. This is not the array alone. We studied the problem of only the array, but when you include the associated central recording stations, logistics, support and transportation you are dealing in billion-dollar figures, in my considered opinion.

Representative HOSMER. These groupings you spoke of adding, were they at these 21 stations, or were they satellites?

Dr. PETERSON. No, they are at the 21 stations. The magnitude of the task was a rather surprising conclusion to us because in commercial prospecting for oil it is quite common and simple for us to "throw out" a number of seismometer groups. Instead of using one seismometer, we use of the order of 30 or 50. It is rather a routine thing to "pick them up and lay them down" several times a day.

But in this situation we are dealing with a completely different order of magnitude of difficulty. We are talking about putting out groups of anywhere from 30 to 100 seismometers over a circular area about 2 miles in diameter. This greatly complicates the problem.

Furthermore, we are talking about doing this over thousands of miles, in wild remote and inaccessible regions, many of them north of the Arctic Circle where temperatures go 50, 60, 70, 80 degrees below zero, where transportation is virtually nil, where you have every known difficulty to overcome. In the construction of the DEW line this was done on home soil, friendly soil, with friendly allies along the Arctic shores where maritime transportation could be had once a year. If this is to be done under less favorable circumstances with unenthusiastic partners, it will greatly compound the difficulties of realizing a successful network.

Representative HOLIFIELD. It would require the transportation in most instances by air, I suppose.

Dr. PETERSON. In many instances, yes. In this study I might mention we investigated, besides the technical factors, the geological and topographic aspects of large-array installation. There was submitted to us an original suggested group of locations at 22 places in the U.S.S.R. which had been carefully picked by a number of experienced seismologists in an attempt to arrange as adequately as possible a network for recording earthquakes and nuclear explosions.

As we studied this preliminary layout, we found out that, though the selection had been made on a rather short notice, it was a very well selected network. The conditions of topography and geography in Russia are such that it is extremely difficult under stipulated conditions to pick many more than 25 good locations. One of the determining factors is that one of the major topographic features in Russia is the Ural Mountain Range. To the east of the Ural Mountain Range there are millions of square miles of wasteland which is nearly inaccessible even to fur trappers. So when you start with the Ural Range as an anchor point and build outward in a network you end up essentially with the kind of network that was proposed. When you locate 22 stations you use up many of the "good" locations in Russia. When you speak of 600 stations you are speaking of geologically low grade and poorly situated stations.

Representative HOLIFIELD. Did you know enough of the topography and geography of Russia to ascertain the exact location of these 21 stations?

Dr. PETERSON. Yes, Mr. Chairman. I would not say exactly, but I can describe very briefly our approach.

As a base map for the study we used a series of charts, world aeronautical charts. An example of one of these charts is shown in figure 2. I think a good deal of this information became available at the time we were allies with Russia during World War II and shortly thereafter. These charts are in many ways surprisingly good. They are, however, deficient in two ways: They are very generalized and the contour intervals representing various elevations are often at intervals of 500 to 1,000 feet, whereas in our study we really need intervals of 100 feet. So we could only estimate.

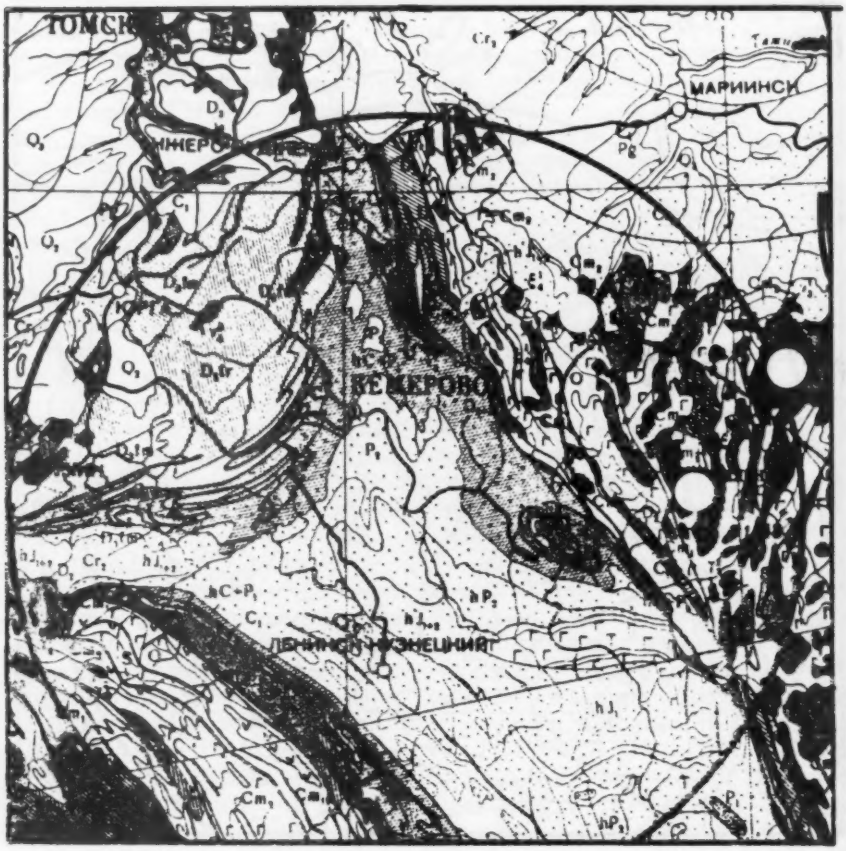
FIGURE 2

Secondly, we made us of rather extensive geological information. The Russians have done, I should say, an outstanding job of mapping the geology of their country. (See fig. 3.) I have heard they put thousands of geologists to work in big lines and "swept" them over the country, and in a relatively short time did a modern job of mapping the country.

Representative HOLIFIELD. Would you say the Russians are further advanced in the art of seismology than we are?

Dr. PETERSON. I won't say that. I would say they have more intensively investigated certain aspects. They have been very much interested in the distribution of earthquakes in highly active regions. Around the margins of Russia in the high Hindu Kush area, which rises to 15,000 or 20,000 feet, and along the Kurile Island chain and Kamchatka, there are very active mountain-building activities now taking place. I think some of the most seismic areas in the world are located in these areas. There are recent publications on detailed studies in southern Russia which show the locations of literally thousands of earthquakes. The maps look like a shotgun pattern at short range. They have done this by concentrating a number of seismological stations in these very active areas.

FIGURE 3



Representative HOLIFIELD. Did you find out any information as to their activity in surveying ocean bottoms?

Dr. PETERSON. Ocean bottoms?

Representative HOLIFIELD. Yes.

Dr. PETERSON. No.

Representative HOLIFIELD. In the field of oceanography?

Dr. PETERSON. We really didn't get into that aspect.

Representative HOLIFIELD. No maps were furnished you as to their work of mapping the bottom of the ocean?

Dr. PETERSON. No. That fell outside of our study project.

Mr. RAMEY. Did you specify the locations of salt domes?

Dr. PETERSON. No, sir. We confined our attention just to the proposed locations of the detection stations themselves. We did look at the proximity to earthquake areas because that is quite an important thing.

Representative HOLIFIELD. We had quite a bit of testimony that the location of salt domes is quite different than your earthquake country. I noticed you said southern Russia had some earthquake areas which I understand is also possibly where they have some salt domes, too.

Dr. PETERSON. I would say this: The earthquake network proposed was laid out mainly with consideration to earthquakes and not salt domes. That might call for reappraisal in some cases. The original network was laid out on the basis of roughly a 1,700 kilometer spacing in nonseismic areas and 1,000 kilometer spacing in the more active earthquake areas. No attention to salt domes was given as far as I know.

Representative HOLIFIELD. In your study of this matter, did you take into consideration the impact that would be upon the program if the land mass of Mongolia and Red China was left out of the station zones?

Dr. PETERSON. Only indirectly, Mr. Chairman. We attempted to make some maps encircling each location with a circle of effective detection distance and we left off the proposed stations in the surrounding areas. The network would be impaired quite seriously if you were not able to fill it out in surrounding countries.

Representative HOLIFIELD. Is it your considered opinion that it would be necessary to have stations in a similar manner throughout the land mass of Red China and northern Mongolia?

Dr. PETERSON. I should say it would be highly desirable and particularly in Mongolia and Afghanistan in the areas bordering the U.S.S.R. on the south, which is one of the very highly active areas. There would be a particular loss if you could not get stations in those areas.

Representative HOLIFIELD. If a station system such as we have talked about were planned, it would take in your opinion from 2 to 3 years to install it, or was that 2- or 3-year period the time period for research and development to develop instruments of five times efficiency?

Dr. PETERSON. Mr. Chairman. I think there would be two steps necessary. First, a preparatory phase where we collect more actual engineering data. Secondly, the actual construction phase covering 3 to 5 years. To date nobody has built a seismometer array or 50 to 100 seismometers. I mentioned the figure of 6,000 to 2,100 miles of high quality cable. We don't know for sure just what the best kind is, the best way to string it. There may be ice and sleep problems. Shall the cable be up in the air, shall it down on the ground? We tried to answer how much shall we use. How many seismometers shall we use? We concluded that the seismometer should be put in a circular area roughly 3 kilometers or 2 miles in diameter. On the basis of our study we recommend that the number be kept down to something like 31 instead of a hundred because of practical difficulties of finding 100 satisfactory locations within an area of a 2-mile circle.

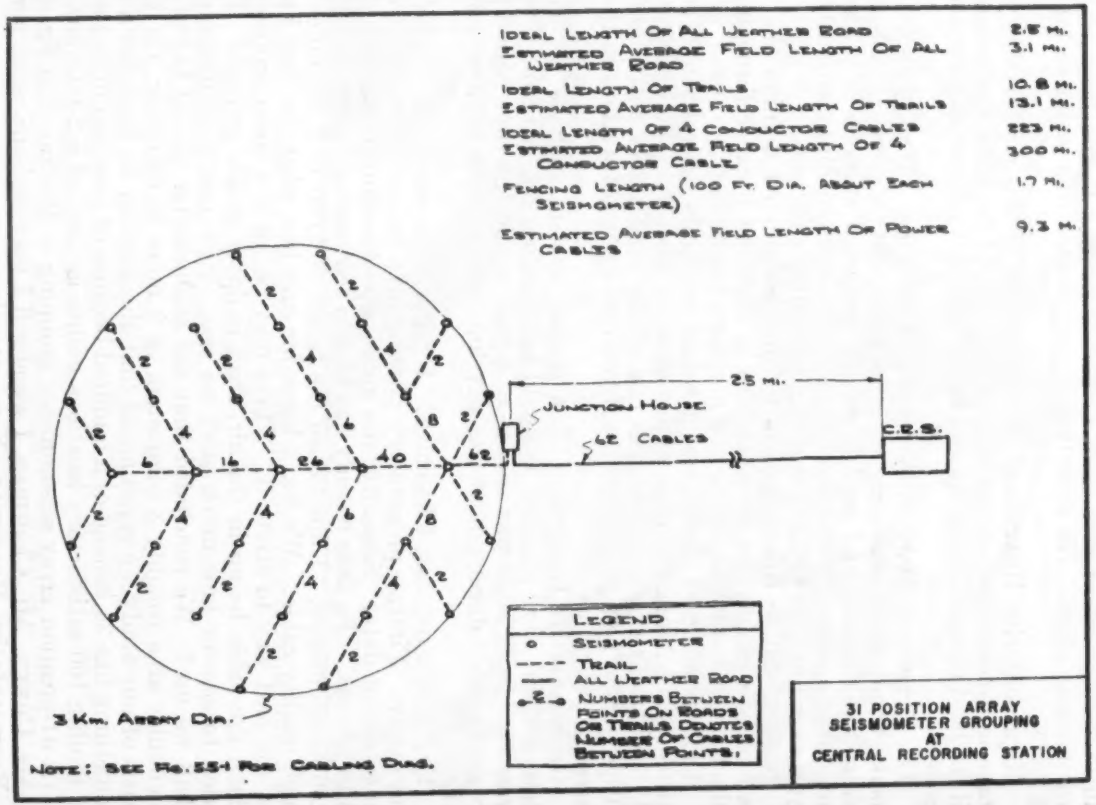
(A 31-position array seismometer grouping is illustrated in fig. 4.)

Mr. COKER. Mr. Chairman, I wonder if I may add one point here.

Representative HOLIFIELD. You certainly may.

Mr. COKER. This question of finding suitable locations for placing seismometers is a serious one. In our opinion it breaks down into two categories: First, it is the matter of finding large basement outcrop areas on which a 3-kilometer circle can be drawn and which are properly spaced at 1,700 kilometers and 1,000 kilometers as previously mentioned.

FIGURE 4


IDEAL LENGTH OF ALL WEATHER ROAD 2.5 MI.
ESTIMATED AVERAGE FIELD LENGTH OF ALL WEATHER ROAD 3.1 MI.
IDEAL LENGTH OF TRAILS 10.6 MI.
ESTIMATED AVERAGE FIELD LENGTH OF TRAILS 13.1 MI.
IDEAL LENGTH OF 4 CONDUCTOR CABLES 223 MI.
ESTIMATED AVERAGE FIELD LENGTH OF 4 CONDUCTOR CABLE 300 MI.
FENCING LENGTH (100 FT. DIA. ABOUT EACH SEISMOMETER) 1.7 MI.
ESTIMATED AVERAGE FIELD LENGTH OF POWER CABLES 9.3 MI.
2.5 MI.
JUNCTION HOUSE
62 CABLES
C.R.S.
6
14
24
40
62
3 Km. ARRAY DIA.
NOTE: SEE RE. 551 FOR CABLING DIAS.
LEGEND
SEISMOMETER
TRAIL
ALL WEATHER ROAD
NUMBERS BETWEEN POINTS ON ROADS OR TRAILS DENOTES NUMBER OF CABLES BETWEEN POINTS.
31 POSITION ARRAY SEISMOMETER GROUPING AT CENTRAL RECORDING STATION

Secondly, within each circle, it is necessary to find isolated locations, a few thousand feet apart, and numbered perhaps 30 or as high as 100, on which individual seismometers can be placed. In the first category, we studied 22 major areas, each 250 kilometers in diameter in an attempt to find whether or not the large 3-kilometer-diameter basic granite outcrops were available. Of the 22 areas, each larger in diameter than the distance from here to Philadelphia, we concluded that in some cases there were no "optimum" geological positions available for these arrays. But to sum it up, from the geology maps, about 75 percent looked reasonable.

The remaining 25 percent fell on poor geology, which would presumably degrade the detection capability of that station, or on very poor geology in some cases. So it boils down to this: To obtain 75 percent out of 22 in these enormous circles, we in several cases had to go outside of the circles by more than a hundred kilometers to find anything suitable. In addition for the remaining 25 percent, nothing really desirable appeared to be present unless very inaccessible locations were selected.

Representative HOLIFIELD. I assume that you have adequate information from these maps to back up the statement which you have just made, we have heard considerable testimony today from Dr. Bethé and others in which they recommend from 125 to 600 stations in the U.S.S.R. Would you say that this is a practical and feasible suggestion that that many stations or areas be found which would allow a battery of seismographs such as 31 to the station to be established?

Mr. COKER. I think I would say this: The probability of finding a larger percentage of, say, 600 high quality locations is much lower than the probability of finding 22. This sounds obvious, perhaps.

Mr. RAMEY. You are talking about different instruments to some extent, are you not?

Representative HOLIFIELD. I want to be sure now I am right in my thinking. Did you understand Dr. Bethé to indicate that the high frequency instruments and the low frequency instruments were needed in the same amounts at each station?

Dr. PETERSON. Mr. Chairman, I was not present prior to this morning and did not hear his testimony of yesterday.

Representative HOLIFIELD. Could I ask for the record at this time if anyone, or Dr. Romney, perhaps, if the same requirement for multiple instruments is needed in the so-called high frequency detection range as in the low frequency?

Dr. ROMNEY. Sir, it would be desirable. But the calculations that have been made assume that you will not have access to those good sites. In other words, Dr. Bethé's calculations do take into account that the sites will be inferior to those being discussed here.

Representative HOLIFIELD. Is that why he wants them placed 200 kilometers apart?

Dr. ROMNEY. That is part of the reason for the smaller spacing; yes. You could increase the spacing of the grid he talked about with fewer stations if you could obtain better sites. No one believes you can obtain such good sites as are being discussed here.

Representative HOLIFIELD. My basic question really was directed to the point of whether you needed as many seismographs at a high frequency installation as you need for the low frequency in order to obtain your average efficiency.

Dr. ROMNEY. No. The proposals made so far call for a single seismograph at each of these 600 stations rather than array.

Representative HOLIFIELD. I am glad to have the record cleared up on that.

Dr. PETERSON. It simplifies some problems greatly if you use only one seismometer. You study in detail the problems involved in installing a 3-kilometer array circle, and you find there are many: to find the right geology, the right kind of rock, not excessive relief. You may even get into other types of problems. For example, what if selected areas fall where there are farmers? Will you evacuate the farmers, will you rerun the roads to avoid noise? You can anticipate more problems in populated zones.

Continuing in this study, since it had a geographical flavor we also made an attempt to study the effect of wind on the performance of the network. This is a very important factor because some of the principal seismic noises in the frequency range involved in these networks is generated by wind. Using available experimental data—by the way, I should say we think a good deal broader range of data would be useful, but we made the best possible use of available data—workong on a relative basis we predict that if under no wind conditions a given station can detect, and I should say that, by "detect" we mean recognize the presence of a signal—suppose under no wind conditions a given station location would "just" recognize a certain size explosion, this would be for only a small part of the year. If you wished to be able to have 50 percent detection time possible during the year it would under similar conditions take 2½ times as large an explosion. If you wished to have detection capabilities equal to no wind conditions, for 80 percent of the year, it would take nine times as large an explosion.

If you wished to have detection capabilities equal to no wind conditions for 90 percent of the year it would take 17 times as large an explosion. At one of the noisy stations, it would take unusually large explosions. So the detection capabilities are very closely coupled to the wind noise.

Representative HOLIFIELD. Then in the place of suggesting these fantastic sizes of weapons which are unnecessary from the standpoint of improvement of weapon capability, conversely if you carried your comparison in the other direction, you would say that a 20-kiloton shot would become less and less detectable as the wind increased.

Dr. PETERSON. Yes.

Representative HOLIFIELD. Could you give us that comparison?

Dr. PETERSON. Yes. Say if you had a 20 kiloton.

Representative HOLIFIELD. We are setting up the stations at 1,700 kilometers.

Dr. PETERSON. Yes. The figures I gave you are relative figures because we geared our study to what relative improvement can be effected by using arrays compared with single seismometers. So the figures I give should not be put in absolute terms. I am trying to illustrate the magnitude by giving ratios.

Mr. COKER. Mr. Chairman, I wonder if I might have the right to put it in a slightly different light.

First, these ratios Dr. Peterson has given are merely illustrations which suggest that an isolated hypothetical station will undergo a reduction in detection capability as the wind velocity increases there.

By limitation of weather and other data, we have had to take the data available and integrate it into a yearly average. In so doing, we have numbers that need to be carefully interpreted. This is an illustration. It is not an absolute estimate of actual performance to be expected at any one of these stations. It is set forth to demonstrate that wind and other local sources of noise may degrade the capability of isolated stations at different times during the year. But it is obviously improbable that all 20 or 22 stations would experience heavy winds at the same time.

Representative HOLIFIELD. It is also improbable that there would be enough bombs set off to register on all the stations, isn't it?

Dr. PETERSON. I am sorry.

Representative HOLIFIELD. Is not the purpose of separating the stations 700 kilometers apart partly for the purpose of making them closer to the location of the explosion and therefore, let us say, in the far eastern side of Russia the seismographic stations there would register more accurately and more strongly, I would assume, than on the European side of Russia?

Mr. COKER. Yes, in part.

Representative HOLIFIELD. If the wind took out across Siberia and affected your signal during a certain period of the year, would it mean that small shots could be set off in that area without detection by those stations in the eastern part of Russia not affected by wind conditions?

Mr. COKER. That is a good strong statement and I am not prepared to completely back it.

Representative HOLIFIELD. That was a question. Far be it from me to be able to make a statement like that.

Mr. COKER. I am sorry. I think the principle is correct. If, near an isolated station, there is a source of local noise, such as a railroad train going by within a few miles, the detection capability of that station is reduced temporarily. The local source of noise might also be the wind. During a gale, maybe the "efficiency" of a particular station is reduced to only 20 percent of its normal value. I think to keep in mind is that any reasonable sized nuclear explosion or earthquake will ordinarily be detected at a number of the stations. In fact, the original concept of the spacing of the stations was to place them 1,700 kilometers a part in zones that are not ordinarily earthquake active and closer, 1,000 kilometers apart, in sesimically active areas so as to more closely monitor these natural events.

Representative HOLIFIELD. Mr. Hosmer.

Representative HOSMER. I guess I do not have any questions. This is a startling bit of testimony here. The best indication is that we are negotiating on the basis of a sham seismological system. The principal value would be to delude yourselves.

Mr. COKER. I would like to add something on a different subject, if I may. One of the basic concepts of this study was to determine the practical value and optimum size of large seismometer arrays as a method for detection and identification. The basic concept is that as you increase the number of seismometers in a geometrical pattern as Dr. Carder pointed out earlier this morning, the ability to detect signals is increased relative to the normal noise that is measured at that station.

Theoretical calculations have been made which suggest very large improvements for such arrays. But these have been done on paper. They suggest improvements of 10 or 20 or more times in the detection capability, by a hundred or 200 or so seismometers.

On the other hand, our study is based on what we believe is practical to put into the field. How many seismometers can physically be located on hard rock? Can they be maintained in northern Siberia? What are the manpower requirements and that sort of thing? Our general conclusion is that, first, some of the mathematically predicted improvements in detection capability of large arrays will probably not be found in actual practice. One of the reasons is that as you put seismometers out on the ground you cannot always place them at the mathematically ideal positions because you do not find rock outcrops there, for example, or they end up on rocks of differing elevations. These practical factors tend to degrade the performance of an actual array somewhat compared to a theoretical mathematical ideal. It should be noted that the practical overall result of fivefold or sixfold improvement for an array of 100 elements confirms the figures used earlier by Dr. Richard Latter in his testimony.

However, combining all practical field factors, we believe that arrays of 100 seismometers may not be the most optimum to use. We certainly believe that arrays of larger than 100 tend to be impractical. In summary an array of perhaps 30 or 50 seismometers may be an optimum value producing an increase in detection capability of about fourfold.

Representative HOSMER. As a practical matter, is the whole thing practical or impractical?

Mr. PETERSON. It is a matter of the effort put in. I think it will take a major crash program and major support to make the thing work and function. It will take great effort. If it is carried out in a wishy-washy fashion it will be a complete "flop."

Representative HOSMER. You spoke of 21,000 miles of cable to feed the information.

Mr. PETERSON. That will be to convey the information from the array area about 2½ miles to a recording station.

Representative HOSMER. Getting information to a station has no value unless you get it out.

Mr. PETERSON. That is right.

Representative HOSMER. Probably it is of little value unless you get it out of all 22 stations into some central place. How much additional cable will that take?

Mr. COKER. This question of communications is somewhat beyond the scope of what we tackled here. I think ordinarily it would be done by other means than cables, such as by sending records, magnetic tapes, or films through the mail, by carrier, or any other form. In addition radio or other communication techniques might be used.

Representative HOSMER. That has to have some degree of integrity.

Mr. COKER. Yes.

Representative HOSMER. You spoke of one of these stations having its seismometers as far apart as from here to Pennsylvania.

Mr. COKER. I must have given an incorrect impression. I mean to say that each of the 22 proposed general locations that we studied was arbitrarily 250 kilometers or 160 miles in diameter. I merely tried to convey a local feeling by saying that this diameter is farther

than from here to Philadelphia. Within each of those areas our objective was to locate favorable access conditions, reasonable topographic and elevation relief, and good geological outcrops of basement rock. In addition it was necessary to hunt for areas which were isolated from local industry which can produce noise. Isolation from railroads, from pipelines, and other manmade noise sources was also required. By combining all of these restrictions and criteria, the conclusion was that it is often difficult, in an area 160 miles in diameter, to locate one or more places where a reasonable compromise exists, that is, fair or good geology, reasonable access to the station and not next to a railroad or a heavy industrial center or too near the seacoast.

Representative HOSMER. You still have about an average of more than a thousand miles of telemetering cables in each one of these stations, haven't you?

Mr. COKER. Yes. May I put it this way: There are two basic size arrays which we considered. One had 31 individual seismometers and the other nearly a hundred, namely, 97. The 31-element array requires 6,600 miles of electric cable for all 22 stations combined or 300 miles per station. The 97-element array requires 21,000 miles of cable for all 22 stations or 1,000 miles per station.

Representative HOSMER. Let me get it this way. Is it in the building or the cable stretched across the country beinging information from one place to another?

Mr. COKER. The cable is deployed over an area roughly 2 miles in diameter connecting to a building that is a few miles away.

Representative HOSMER. That cable is subject to sabotage and any other thing that might happen to it by man, in addition to the elements; is that right?

Mr. COKER. Presumably it would be.

Representative HOSMER. Then you still have a problem of radio or some way of getting the information that is out in one of these stations to a central area?

Mr. COKER. That is correct.

Representative HOSMER. Your study did not encompass the protection of the integrity, either, of the communications within the station nor between stations and central headquarters?

Mr. COKER. It did not.

Representative HOSMER. Thank you.

Representative HOLIFIELD. Any questions, Mr. Bates?

Representative BATES. Wouldn't it be possible to nullify the effects of all of these stations simultaneously by noise?

Dr. PETERSON. I believe it would be difficult, sir, to do this by artificial means. Theoretically you could set up shakers near every station but it would be a very difficult and crude attempt. I think the most practical use of noise would be to make use of earthquake noise, though there are many difficulties. It is conceivable that an underground explosion could be triggered in the wake of a natural earthquake occurring within a hundred kilometers and within a half minute or so before. This approach would give a "noise shield" so that the whole thing would be shielded at a good many stations.

Representative BATES. With a combination of wind and other factors could you pretty well eliminate picking up a signal that you could identify?

Dr. PETERSON. I would say that the destruction of detection capabilities at all stations simultaneously is very improbable, but degradation at a few critical stations could vitiate that part of the network. I think the more practical means or the more real danger would occur if the explosion were deliberately set off in the wake of a fairly good-sized natural earthquake, of which there are a substantial number occurring every year. That would be another way of attempting concealment.

Representative BATES. When you have one of these first motion indications you could let it go then.

Dr. PETERSON. Within a half minute or so after you have identified it.

Representative BATES. So even with this tremendous effort and expense you still wind up with any kind of certainty even with decoupling.

Dr. PETERSON. That is right. You end up with a very low probability of detection if there is an intent to concealment. You have very little chance of detection.

Representative HOSMER. Right along that line, with this system that you have described here, if you were calculating whether or not to try a clandestine test, and you used your best efforts at concealment, what chance do you think you have of beating the system?

Dr. PETERSON. I hesitate to put numbers but maybe 99 chances out of 100 if you went at it in a competent fashion.

Representative HOSMER. Ninety-nine chances out of a hundred?

Dr. PETERSON. Yes; by evasion, by using proper decoupling and timing.

Representative HOLIFIELD. Does that end your testimony?

Mr. COKER. I have another point or two if I might add it.

Representative HOLIFIELD. We would be glad to have it.

Mr. COKER. One thing which we did not completely expect in this study, a factor that came up as we went along was the need to accurately know the geographical location of each of the stations which you put in a foreign area. This sounds so obvious, but it is not obvious. If you place a station at some spot out in the U.S.S.R., all of the readings that station makes and all of its predictions of the locations of earthquakes or bomb tests are no better than the accuracy of the station location. So the problem that arises is twofold. First, you must accurately survey the precise location of each of the 22 stations so that they are precisely known on a global longitude and latitude network.

Secondly, in order to direct an inspection team to the spot on the physical ground they desire to inspect, you must have sufficient local mapping so that when they get there, they know how far to go and in which direction and where to stop and where to start looking.

I hope I am making this clear. There are two kinds of different problems. One is that you have to make 22 accurate surveys, and this may be relatively simple. The other one is, that you must be able, by more accurate topographic maps or portable surveying apparatus or by some other means, to direct the inspection team so that they can find the area on the ground that all the detection stations located. I am not aware of the degree of accuracy of surveying in the U.S.S.R. at the present time. We understand there is a fine triangulation net over certain parts, but the question of local surveying accuracy should be an understood limitation of the system.

Representative HOLIFIELD. And this same requirement would be applicable to the land mass of Red China, India, Africa.

Mr. COKER. Presumably.

Representative HOLIFIELD. And the Western Hemisphere.

Dr. PETERSON. Mr. Chairman, many of the inaccessible parts of the world have been located merely by taking astronomical observations. We have not made a thorough study but we suspect that the locations of many of these spots are not known within 25 miles at the present time. It would take a very all-out method to get a good base map on which to work out the area. There is a major job there involving many man-years of effort.

Representative HOSMER. I think that should be brought out with clarity. You might locate each of these 22 stations in latitude and longitude, but still you have to have the rest of the country covered accurately in order to make this meaningful, is that correct?

Dr. PETERSON. Yes. You might get out there and you might be 10 miles farther than you think you are. You would be no closer than the map accuracy.

Representative HOSMER. And that throws all the rest of the locations off.

Dr. PETERSON. That is right.

Representative HOSMER. Incidentally, when you got all that, the chances are still 99 in 100 that you mentioned before?

Dr. PETERSON. I discounted a lot of these things in making that statement.

Representative HOLIFIELD. Gentlemen, you have certainly given us food for thought today, and we very much appreciate your presentation.

We will resume at 10 a.m. in the morning in the old Supreme Court room, with Dr. Byron P. Leonard from Space Technology Lab, Los Angeles, and the balance of the witnesses.

(Additional material submitted by Dr. Peterson and Mr. Coker follows:)

ABSTRACT OF TECHNICAL STUDY REPORT ENTITLED "INVESTIGATION OF PROPOSED USE OF LARGE SEISMOMETER ARRAYS IN THE U.S.S.R. FOR THE DETECTION OF UNDERGROUND NUCLEAR EXPLOSIONS"

(This abstract is supplied in support of testimony given by Dr. Raymond A. Peterson and Frank B. Coker before the Joint Congressional Committee on Atomic Energy, April 21, 1960)

ABSTRACT

The report deals with five general subjects:

1. The characteristics and practical effectiveness of large seismometer arrays.
2. The availability of suitable sites for 22 large array installations in the U.S.S.R.
3. The effects of natural and man-made noise sources and earthquake activity on the spacing and efficiency of the stations.
4. The cost of installation and operation of only the large seismometer arrays at the 22 proposed station locations, exclusive of costs of other essential portions and features of the overall seismic detection system.
5. The importance of adequate provisions for an accurate base map of the U.S.S.R., realistic testing of the finished network and reasonable flexibility in its operation.

The results of the study suggest—

1. Large array characteristics

(*a*) Performance of actual seismometer arrays is degraded compared to idealized arrays owing to practical field factors including—

(1) Deviation of actual seismometer positions from ideal positions.
(2) Deviation of actual seismometer elevations from ideal.
(3) Variations in geological environment.
(4) Omission of seismometers from arrays, caused by natural obstacles.

(*b*) The effects of these "natural" factors were simulated in this study by a series of digital computer calculations. The results include an estimate of relative improvement in performance to be expected from various size arrays, together with a set of criteria for field placement of large arrays.

(*c*) These criteria were applied to 2 basically different size arrays containing 31 and 85 elements, respectively. The resulting expected improvement compared with a single seismometer was—

Thirty-one elements, 4 times improvement.
Eighty-five elements, 5.5 to 6 times improvement.

(*d*) Although the degree of improvement increases with the number of seismometers, a stage of diminishing returns is reached owing to—

(1) Unavailability of geologically suitable positions for every seismometer.
(2) Burdensome field maintenance requirements.
(3) Decrease in seismic efficiency caused by close spacing of seismometers.
(4) Excessive installation time required in view of adverse weather conditions and short working seasons in northern areas resulting in serious delays in putting the overall system in effective operation.

After careful weighing of all these factors, it is recommended that approximately 31 seismometers be utilized in each array.

2. Availability of sites

(*a*) The selection of suitable sites for installation of large seismometer arrays involves a compromise between the following factors:

(1) Availability of sites geologically favorable to seismic efficiency:
(a) Each station requires a granitic or metamorphic rock outcrop 3 kilometers (2 miles) in diameter.
(b) Within each 3-kilometer circle must be found properly spaced individual rock outcrops for each seismometer.
(2) Reasonable topography allowing acceptable seismometer emplacements without excessive elevation differences.
(3) The array sites must be spaced sufficiently far from natural and manmade seismic noise sources including railroads, pipelines and heavy industry.
(4) Reasonable accessibility.

Final site selection requires on-the-ground inspection. However, preliminary selections can be made from studies of geologic and topographic maps.

(b) These criteria were applied to 22 locations in the U.S.S.R. spaced in proper relation (1,000 kilometers or 1,700 kilometers). Each study area was 250 kilometers (156 miles) in diameter. The results for two of the most important criteria, geology and accessibility, are summarized below:

	Number of stations	Percent of total
Good geology:		
Good access	[1] 9	41
Fair access	5	23
Poor access	2	9
Fair geology: Good or fair access	2	9
Poor geology: Good or fair access	3	14
Very poor geology: Fair access	1	5
Total	22	

[1] 2 are located considerably outside of the originally suggested 250-kilometer diameter circles.

In summary, 73 percent of the stations could apparently be put on good geology with good, fair, or poor accessibility. The remaining 27 percent fall on fair, poor or very poor geology with consequent reduction in their detection capability.

In addition, 27 percent of the locations are situated near large bodies of water which may result in higher than average microseismic background noise levels. Two fell in the category of good geology; four fell in the areas of less desirable geology.

3. Noise sources, earthquakes, and network coverage

(*a*) In most cases locations could be found which minimized effects of manmade noise.

(*b*) Wind noise may drastically degrade individual station performance, but the effect is presumably localized to only a few stations at a time corresponding to local weather conditions. On the other hand, one or two "wind degraded" stations may seriously reduce the network's capability to identify events using the polarity of first motion criterion. For an average station, the relative increase in nuclear yield involved over that required for detection in the absence of wind is—

70 percent for detection 50 percent of the time.
370 percent for detection 80 percent of the time.
750 percent for detection 90 percent of the time.

(*c*) Spacing of the proposed 22 locations corresponds closely with the areas of high and low earthquake activity. However, U.S.S.R. coverage will be considerably impaired if not supplemented by additional stations outside the borders, especially to the south.

(*d*) Stations near the areas of high earthquake activity will have a very heavy data handling workload with need for correspondingly increased numbers of operating personnel and equipment.

4. Cost to install and operate arrays

(*a*) A detection station comprises two basic parts:

(1) Seismometer array itself.

(2) Associated recording and communication equipment, buildings, access roads, even airfields in some cases.

(*b*) The following approximate cost estimate covers only item (1), the seismometer array itself. It applies to 22 stations.

Estimated installation cost for twenty-two, 100-seismometer arrays (not including station recording and communication equipment, buildings or access roads), $130 million.

(*c*) Estimated annual operating cost for the arrays only is $7,500,000.

(*d*) At this very early stage there were a great many important factors which could not be determined. For example: Who will do the actual work? Where will the equipment and cables be constructed? Will construction be on a crash basis, or will it spread out over a number of years? How will equipment and supplies be transported? All these factors have a major effect on the final costs of the actual program. The cost estimates given in the report are therefore to be considered as strictly minimum values. To them must be added the cost of the recording stations (electronics equipment, communications, support, and living facilities) plus the cost of constructing access roads and even airfields in some northern stations. It is important that a detailed and accurate cost estimate be made including all these and other applicable factors.

5. Factors essential to network operation

(*a*) A high precision base map of the U.S.S.R. is essential to useful application of network detection data.

(*b*) Realistic testing of the finished actual network with an adequate number of controlled underground explosions is vital in evaluating the effectiveness of the overall network.

(*c*) Careful advance planning and negotiations are essential in providing reasonable flexibility in constructing and operating the network:

(1) Multinational construction and operating teams will probably operate at reduced efficiency owing to language differences and differences in living habits, approaches to technical problems, etc. Methods must be established for resolving technical and human problems which arise.

(2) Operating agreements must be established enabling reasonably frequent visits by supervisory personnel of the same nationality as the resident station personnel in order to preserve their morale.

(3) Agreements must provide for a reasonable frequency of local leave and rotation and replacement of personnel.

(Whereupon, at 5 p.m., Thursday, April 21, 1960, the committee was recessed, to reconvene at 10 a.m. Friday, April 22, 1960.)

TECHNICAL ASPECTS OF DETECTION AND INSPECTION CONTROLS OF A NUCLEAR WEAPONS TEST BAN

FRIDAY, APRIL 22, 1960

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION,
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittees met, pursuant to recess, at 10 a.m. in room P–63, the Capitol, Representative Chet Holifield (chairman of the Special Subcommittee on Radiation) presiding.

Present: Representatives Holifield, Durham, Hosmer, and Bates.

Also present: James T. Ramey, executive director; John T. Conway, assistant director; George F. Murphy, professional staff member; and Richard T. Lunger, staff consultant.

Representative HOLIFIELD. The committee will be in order.

The Special Subcommittee on Radiation and the Subcommittee on Research and Development resume this morning the public hearings on the technical problems of a nuclear weapons test ban and the necessary detection techniques and inspection controls.

We are in the fourth full day of these hearings. We have been very fortunate in having as witnesses a number of highly competent scientists who have given freely of their time to assist this committee in understanding the problems we face.

While there has not been unanimity in all areas of the testimony so far, we are, I believe, succeeding in narrowing areas in which there are differences. The manner in which our scientific witnesses have been presenting their divergent views and their willingness to sit together around a table in discussion, as was done yesterday in our panel meeting, speaks well for the sincerity and objectivity of the scientific community as represented by our witnesses.

I believe I can speak for the members of both subcommittees, as well as all members of the Joint Committee who have been attending the hearings, in saying we are deeply appreciative of the fine public service our witnesses have been performing.

This morning we will resume with a discussion of the Geneva-type international control system with regard to the problem of detecting high altitude and space tests. Our first witness will be Dr. Byron P. Leonard, Space Technology Laboratory, Los Angeles, Calif.

Dr. Leonard will be filling in for Mr. Allen F. Donovan of the Space Technology Laboratory and is prepared to discuss the satellite vehicle requirements for reporting the system.

He will be followed by Dr. Richard Taschek of the Los Alamos Scientific Laboratory, who is prepared to discuss the period of time it will take for achieving capability in this field.

Dr. Leonard, we will begin, if you are ready.

STATEMENT OF ALLEN F. DONOVAN,[1] SPACE TECHNOLOGY LABORATORY, LOS ANGELES, CALIF., READ BY DR. BYRON P. LEONARD [2]

Dr. LEONARD. Thank you, Mr. Chairman.

Gentlemen, Mr. Donovan, whom you invited to testify on satellite vehicle requirements for systems for detecting nuclear explosions in space, has asked me to express his personal regrets that he is unable to be here today. Other pressing matters made it impossible for him to attend on such short notice, but he has prepared a statement and delegated me to present it to you.

My name is B. P. Leonard, and I am project engineer on the studies being pursued by the Air Force Ballistic Missile Division and Space Technology Laboratories on systems for detecting high-altitude nuclear explosions.

This work is being done under the direction of the Advanced Research Projects Agency and in cooperation with the Atomic Energy Commission and the National Aeronautics and Space Administration.

It entails systems analyses of the satellite-borne detection systems proposed by the Geneva Working Group (Technical Working Group I) in July of 1959. It is expected that the results of these studies will lead to a better understanding of the capabilities, requirements, and costs of the proposed systems.

I will now read Mr. Donovan's statement.

Gentlemen, I regret that due to a longstanding commitment I am unable to be present today to personally present my opinions on space vehicle requirements for systems to detect nuclear explosions in space. I have, however, prepared this written statement for your committee.

In addition, I have arranged for one of my colleagues, Dr. B. P. Leonard, who has worked with me on studies of systems for the detection of nuclear explosions in space, to be present to answer questions and present such additional material as your committee may require. Should there be further interest in this general subject,

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[1] Position: Vice president of Space Technology Laboratories, Inc., and Director of Advanced Systems Planning Division.

Education: Received his B.S.E. and M.S. degrees in aeronautical engineering from the University of Michigan.

Experience: His background of experience includes positions both in the aircraft industry and with the university-operated Cornell Aeronautical Laboratory. His work has involved a wide range of research and development activities on manned aircraft, guided missiles, and space vehicles. During his 4 years with Space Technology Laboratories, Inc., and its predecessor, the Guided Missile Research Division of the Ramo Wooldridge Corp., he has been in charge of the technical direction of the rocket vehicle aspects of the Air Force ballistic missile program, including particularly the propulsion and nose cone developments for the Atlas, Titan, and Thor missiles. In addition he served as Titan program director in the early phases of the Titan missile development, contributed to the establishment of the solid propellent Minuteman missile program, conducted a series of space vehicle studies which led to the Air Force Pioneer lunar probe program, and supervised the technical activities related to the vehicle aspects of the Air Force Pioneer program. He served as a U.S. delegate to the Conference on Nuclear Test Suspension held this past summer at Geneva, Switzerland. Currently chairman of the Propulsion Panel of the Air Force Scientific Advisory Board, Mr. Donovan is also coeditor with H.R. Lawrence of vol. VII of the Princeton series on high speed aerodynamics and jet propulsion, and the author of a number of technical articles.

[2] Bachelor's degree in physics from Southwestern Louisiana Institute and a master's and doctor's degree in nuclear physics from the University of Texas.

After obtaining his bachelor's degree in 1943, entered the U.S. Navy where he served until 1946. After his discharge he became a project engineer at the U.S. Naval Ordnance Test Station where he was responsible for range test firings of experimental rockets from aircraft.

He then became a member of the physics teaching staff at Southwestern Louisiana Institute. In 1950 he entered the University of Texas where he taught physics, carried out nuclear research for the Air Force, and subsequently received his doctor's degree.

In 1953, joined the Convair Aircraft Co. on the aircraft nuclear propulsion program. There he became head of the nuclear aircraft research facility where two experimental reactors were operated to obtain data on nuclear shielding and radiation effects to aircraft materials and components. In this capacity he was responsible for the nuclear test airplane program which entailed first flights of a test reactor aboard a B-36 airplane.

Since joining Space Technology in 1959, has been responsible for studies on advanced nuclear systems including possible detection systems for high-altitude nuclear explosions.

I would welcome the opportunity to appear before your committee at future hearings.

For your general information, my field of technical specialization lies primarily in the design and development of space vehicles, particularly those aspects related to propulsion, structures, guidance and control, and communications.

I served as a member of Dr. Panofsky's delegation (TWGI) to Geneva with the assignment of providing information in the satellite and space vehicle area. By training I am an aeronautical engineer, not a physicist, and my present position is vice president and director of the Advanced Systems Planning Division of Space Technology Laboratories, Inc., in Los Angeles, Calif.

You probably know Space Technology Laboratories, or STL as it usually called, as the organization responsible for the systems engineering and technical direction of the Air Force's Atlas, Titan, Thor, and Minuteman ballistic missile programs. In the space field, it was STL who built and launched the space probes Pioneer I, II, and V, and the satellite Explorer VI for NASA under the management of the Air Force Ballistic Missile Division.

I have tried to anticipate the areas of interest to you in which I might supply information. I believe that there are primarily two: First, the ability to conduct nuclear tests in space; and second, the capability of our vehicles to install systems in space for the detection of nuclear explosions.

Because of limitations of security on the capabilities of our space-launching vehicles and related items, I will confine my remarks primarily to those items discussed at Geneva or which have previously been released.

First, let me discuss the ability to conduct nuclear tests in space. Perhaps with the achievements of both the Soviet Union and ourselves in space flight, this topic does not need clarification. However, to be sure that there is no question about it, let me state simply that a vehicle such as the Atlas ICBM, with appropriate upper stage rockets could launch a nuclear device—together with sufficient instrumentation and radio equipment for a test—to a velocity above that required for escape from the earth.

Radio communication with this test package and tracking of it in space could be accomplished as is being done with the space probe Pioneer V, which is currently relaying information to us from a distance in excess of 5 million miles. When the device had reached the desired distance, which could be in the range of tens of millions of miles, a radio command would separate the instrument package from the bomb.

After a preset interval necessary to provide the desired separation, a timer would detonate the test device while the instrumentation recorded the pertinent data and radioed it back to Earth. When space vehicles of the future, with bigger payloads, come into being, large weapons could be tested in space.

Furthermore, part of the payload could be used to put a shield around the device to make it more difficult to detect the explosion. Shielding might also be accomplished by testing behind the Moon, for example. The Soviets have already demonstrated with their Lunik III the ability to go behind the Moon, and their demonstrated superior payload capability would give them an advantage over us in conducting such tests at the present time.

Let me comment briefly on the safety problem. Many uninformed people have expressed concern over the possibility that the failure of a rocket vehicle during launch of a test device could result in a major catastrophe. I would like to say merely that it appears that the danger of such an accident producing a nuclear explosion in the atmosphere or on the Earth can be adequately guarded against through techniques already developed.

Therefore, let me summarize with the statement that testing in space is feasible even at distances up to tens of millions of miles and in such regions as behind the Moon.

Representative HOLIFIELD. Is it not true, Dr. Leonard, that we are talking about a program here which is pretty much in the future when you consider the capability of providing vehicles with enough accuracy of guidance and the telemetering system, the coordination that has to occur between the test vehicle itself and the recording vehicles and the scope of the recording vehicle's capability, the telemetering back to Earth and the reception of the information needed.

Is that not a pretty far-fetched procedure that you are talking about here?

Dr. LEONARD. No, sir; I think not.

I would say that all the basic technology required for such a test is available today. I think, too, that in general the vehicle capabilities are such that a nuclear test could be run in space with present capabilities.

Representative HOLIFIELD. At what cost in time, effort, and in dollars?

Dr. LEONARD. I believe as far as time is concerned it is clear that the test could be run before a full-blown nuclear detection system could be put into space.

Representative HOLIFIELD. Would you define that in terms of years?

Dr. LEONARD. I would say it is just a matter of less than 2 years that would be involved after it was decided to perform a nuclear test.

Representative HOLIFIELD. What would you hope to learn in a test of that type? What types of information could you get back?

Dr. LEONARD. As far as the kinds of information that could be gotten back, the technology for transmitting data today is entirely adequate for getting information back. As far as detailed measurements to be made, I do not think I am qualified to answer that part of the question.

Representative HOLIFIELD. I know you can get the information back that the device had exploded. Could you get the information back as to the power of explosion?

Dr. LEONARD. Yes, sir; I think that is probably one of the things that can most readily be done.

Representative HOLIFIELD. You think that could be done?

Dr. LEONARD. Yes, sir. The question of other details on the performance of the device gets progressively more difficult and this is an area that I am not too familiar with.

As far as the yield of the device, that is apparently one of the things that could most readily be done.

I will now continue with Mr. Donovan's statement.

Let me next proceed to discuss the satellite vehicle requirements for space detection systems. The principal systems discussed by the

High Altitude Technical Working Group at Geneva (Technical Working Group I) were a far-earth satellite system, a solar satellite system and an Argus satellite system.

In addition, a near-earth satellite system was discussed as an alternate to the far-earth system if launching vehicle limitations made the far-earth system too difficult or expensive.

The far-earth satellites were conceived as operating in orbits from 30,000 to 50,000 kilometers altitude, or roughly 20,000 to 30,000 miles high. The system would consist of from four to six satellites arranged in orbits so that the detectors would survey all the space below them down to the surface of the earth's atmosphere and all space outward from their orbit except for the regions shielded from view by the moon, the sun, and other planets.

To cover these regions and to assist in the detection of shielded explosions at large distances, the U.S. delegation proposed a system of solar satellites. The Soviet Union objected to adding these vehicles for detecting explosions behind the moon in spite of the fact that the Soviet luniks demonstrated the capability of reaching this region. The Soviets took the position that the cost of launching solar satellites would be excessive and the communications problem too severe. Nevertheless, they were willing to accept the far-earth satellites in orbit at 50,000 kilometers. Strange as it may seem to those of you who have not been involved in the peculiarities of space vehicles, it takes a more powerful rocket system to put a satellite in orbit at 50,000 kilometers altitude than it does to launch one above the speed of escape into orbit around the sun. In either case, it requires a powerful multistage rocket system beginning with an Atlas or larger size booster.

Fortunately, the development of space boosters by the Air Force and NASA will make launching vehicles available sooner than it appears that satellite payloads for detection can be developed.

With respect to the satellite vehicle itself, suitable radio communication, tracking and control links, together with their space power supplies, have been developed and are being tested in Pioneer V. Devices for storing data and playing it back on command are in use.

The temperature control system for Pioneer V is functioning perfectly. Techniques for building lightweight satellite structures are available, together with the means for properly stabilizing the satellites in orbit. Designs for instruments to detect the basic signals from a nuclear explosion have been proposed by Drs. Panofsky, Bethé, Latter, and others.

Development of these sensing instruments and the electronic analyzer which will discriminate between the signals from a nuclear explosion and the natural radiation in space must be accomplished. Then detection instruments, analyzer, data storage, communication link, stabilizing system, temperature control system and surrounding structure must all be carefully integrated into a total satellite vehicle system matched to the launching vehicle.

Flight trajectories must be worked out to place the satellite in the proper orbit while adhering to the requirements of range safety. Tracking and receiving stations must be established at appropriate points around the earth to command the satellite and receive its data. These must be suitably connected to a central command center and a data processing and analysis center must be provided to interpret the satellite's output.

Fortunately, the Air Force is currently establishing under other programs a worldwide network of tracking stations of the type necessary to serve nuclear detection satellites. Thus, the technology and procedures for creating a nuclear detection ground system will exist in the near future.

Work on other U.S. space programs sponsored either by the Air Force or the NASA is providing subsystems and technology as outlined previously for all aspects of the satellite vehicle except the basic nuclear signal detectors and the signal analyzer. Availability, therefore, of a first exploratory nuclear detection satellite is limited technically, first by the availability of the detectors and the associated analyzer, and secondly by the time to integrate these units together with the storage system, communication system, power supply, temperature control system, and stabilization system into a total satellite vehicle.

This time is sufficient for procurement of launching vehicles. Launch-stand availability does not appear to pose a problem for the initial exploratory satellites. It will, of course, depend upon the number of other space launches planned and the relative priority assigned to the nuclear detection program.

Before closing, I would like to insert a word of caution so that the preceding statements on satellite vehicle development will not be misinterpreted. There are important questions as to the effectiveness of satellite-based systems for the detection of nuclear explosions at high altitude which can only finally be resolved by measurements of the natural radiation in space and testing detection system components in this radiation environment.

I wish to make it clear that our present concept of a satellite detection system assumes certain things about this natural radiation. Only after this background is known and experience is gained from testing detection equipment in it can we accurately assess the effectiveness of any satellite system proposed. Hence, my statements on the ability to create and launch satellite vehicles of the presently conceived type while certainly encouraging, do not automatically insure that doing so will give us an adequate capability for detecting nuclear explosions in space. I am sure that Dr. Taschek and others will expand on this point in their testimony.

This concludes my prepared statement on the matters related to satellite vehicle requirements for nuclear detection systems. I would like to thank you for this opportunity to present my views on this subject and to again express my willingness to appear before your committee in the future.

Mr. Chairman, I have a few informal remarks which I think might add to the testimony as to what the present situation is on nuclear testing and detection.

Representative HOLIFIELD. Proceed.

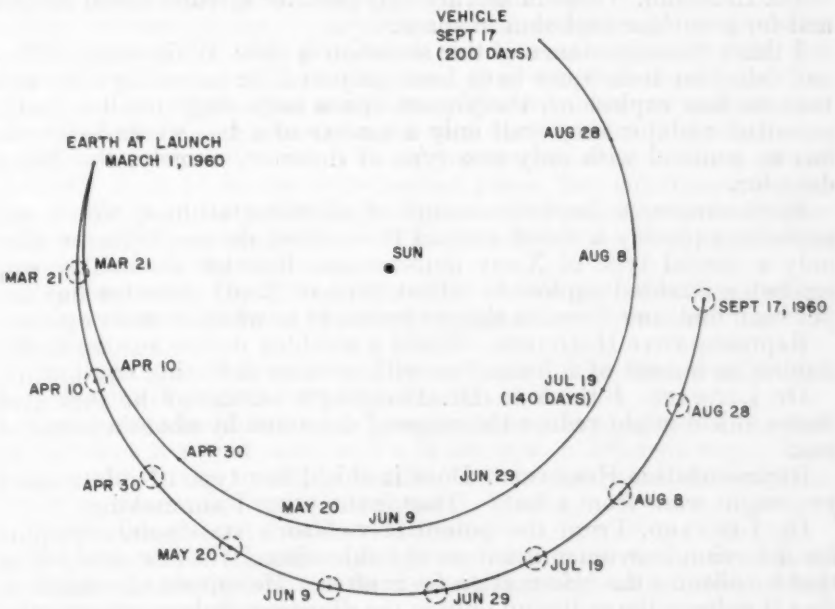
Dr. LEONARD. I believe that Dr. Panofsky, in his testimony, showed information on the ranges of various detections systems for detecting high altitude nuclear explosions. I would like to show this kind of information on a different scale; namely, on the scale of solar system distances.

I think it helps to put the true detection situation in its proper perspective. First of all I have taken some numbers from the report of the working group in Geneva, TWGI, and scaled them up to ranges for the various detection techniques for a 1-megaton device.

I have used a megaton here because I think it is clear that for very large-yield weapons there is a high probability that a potential violator would tend toward a space experiment rather than one under or near the earth's surface.

Scaling those numbers up, the implications are that a gamma ray detector might have a range on the order of 5 million nautical miles. For a neutron detector, its range would only be of the order of 2 million nautical miles. I think Dr. Taschek will present more recent figures which are somewhat larger than these but the differences do not have a material effect upon the arguments which follow.

In the case of the X-ray detector, the working group stated that for a 10-kiloton device, the range would be several hundred million kilometers. I think this implies a rather large uncertainty on just what the range of the X-ray device will be. I have interpreted this situation by using two numbers, one of 500 million miles range and one of 5 billion, which I think brackets the present theoretical understanding of the range of the X-ray detector.



Nuclear Detection Ranges and the Pioneer V Orbit

In the figure shown here I have attempted to show the earth's orbit around the sun, with a diameter of the order of 160 million nautical miles. If we place a circular dot around the earth of a radius 5 million miles, which appears to be somewhere near the upper limit of all the detection ranges for other than the X-ray device, we see that the ranges of all other detection systems are quite small as compared to the diameter of the earth's orbit around the sun.

On the other hand, for the X-ray device, the theoretical predictions are that it can see a nuclear explosion at a distance as great as 3 to 30 times that of the diameter of the earth's orbit around the sun.

So you see there is a tremendous difference implied here between the X-ray detection capabilities and all others.

Now, to place this in its proper perspective, I have shown here also the orbit of Pioneer V, which is presently in orbit and sending data back to us. Its orbit is such that at 3 weeks it reaches a distance of about 4 million nautical miles from the earth. At 7 weeks, 8 million miles; 11 weeks, 18 million, and finally, at about half a year it will have reached 85 million nautical miles distance away from the earth.

Representative DURHAM. Would that X-ray statement be true if you had an underground explosion at 1,000 feet underground?

Dr. LEONARD. No, sir.

Representative DURHAM. This applies only to satellites and explosions in the air?

Dr. LEONARD. In space. As a matter of fact, in air the range goes down tremendously due to the attenuation of air.

Representative DURHAM. It would not apply to underground detection?

Dr. LEONARD. These ranges are only good for satellite-based devices and for a nuclear explosion in space.

I think the importance of this situation is this: While many different detection techniques have been proposed for detecting high altitude nuclear explosions, the present space technology implies that a potential violator need wait only a matter of a few weeks before he has to contend with only one type of detector, namely, the X-ray detector.

Furthermore, a limited amount of shielding studies, where one postulates placing a shield around the nuclear device, indicates that only a special type of X-ray detector can function reasonably well against a shielded explosion. That type of X-ray detector has not yet been built nor flown in the environment in which it must operate.

Representative HOLIFIELD. Would a shielded device automatically reduce its output of information with present detecting equipment?

Dr. LEONARD. I think in Dr. Panofsky's testimony he indicated that a shield might reduce the range of detectors by about a factor of ten.

Representative HOLIFIELD. Does it shield the type of information you might want from a test? That is the point I am making.

Dr. LEONARD. From the potential violator's standpoint, he puts his detection instrumentation on the side where it is not shielded so that he obtains the information he is after. He orients the shield so that it reduces the radiation only in the direction of the nuclear explosion detection system. So now it is a question of whether the detection system detects the signal at all. If the signal is detected, presumably the system will have enough capability in the detector to determine whether it was a nuclear device or some natural phenomenon. So I would say from the detection standpoint it does not necessarily degrade the information. It is just a question of whether you receive it or not.

Representative HOSMER. You said this device has not been created yet or tested. How long would it take to create such a device?

Dr. LEONARD. I think that is a rather difficult question since the detection device which has been proposed by Dr. Panofsky requires some basic research and development.

Representative HOSMER. Then there is a period of years when testing could be carried on in space without detection?

Dr. LEONARD. That is true.

Representative HOSMER. With a 100 percent chance of being undetected.

Dr. LEONARD. That is right. It is really no trick at all to run tests outside of the X-ray detection circle shown in the figure.

Representative HOSMER. Yesterday we had testimony that you could do it underground with a 99 percent chance of not being detected. This is an even more loose area.

Dr. LEONARD. That is right. I think, however, that was predicated upon getting a certain capability in these ground networks.

Representative HOSMER. They would have to improve tremendously to get that one-in-a-hundred chance.

Dr. LEONARD. I would like to conclude my remarks by simply stating that it is clear that in the area of space, detection systems are in much the same situation that we have been previously on underground shots, namely, we are relying upon the development of a new device to detect nuclear explosions at great distances from the earth.

The uncertainties in just what the ultimate effectivity of this detector will be are such that it is not clear what the final capability of a space detection system will be. The only way we can make it clear is through a vigorous research and development program.

Because of this situation, I think you can see that we must pursue not only R. & D. in the underground phase, but simultaneously on the problems of detecting high altitude nuclear explosions. Otherwise, we will be in the position of having closed the front door of the barn but left the back door open.

Representative HOLIFIELD. The cost of this particular type of research and development, of course, is beyond your comprehension at this time?

Dr. LEONARD. We have done some work in this area and I think Mr. Beyer will present some information on it.

Representative HOLIFIELD. I am not sure you answered a question on the time it would take, so I will ask it in a different way. How long would it take to set up the first test to determine the ability to detect nuclear weapons in outer space?

I am talking about a calibrated shot.

Dr. LEONARD. If I understand your question correctly, sir, I think it implies that a nuclear test is required to check out a nuclear detection system; is that correct?

Representative HOLIFIELD. It might be. Otherwise it would be an extrapolation from a chemical test. There is no prohibition against having chemical tests in the atmosphere that I know of.

Dr. LEONARD. I think among many people who have looked at this part of the problem there is a feeling that this system might be set up with a reasonable understanding of its capability without running a nuclear test.

Representative HOLIFIELD. There is no present prohibition that I know of against any kind of chemical test in the atmosphere.

Dr. LEONARD. The chemical tests would be useless in providing information on the capability of this system, since it is predicated only upon measuring nuclear radiations.

Representative HOLIFIELD. You are talking about a system. But the reason for developing this type of a system would be to have a surreptitious or concealed test of a nuclear weapon.

Dr. LEONARD. I think probably we are talking about two different aspects.

Representative HOLIFIELD. Your X-rays and your neutrons and your gamma rays will come from a nuclear explosion and you would have to have that kind of explosion in order to test detection instruments of that kind. You could not get it from a chemical explosion?

Dr. LEONARD. This is true.

Representative HOLIFIELD. If you are talking about putting a satellite there, surely you can do that. We are doing that anyway. I am talking about tying up an orbiting satellite with a nuclear explosion and nuclear detection instruments.

Dr. LEONARD. I see. Let me state what probably would be two different points of view on this particular subject.

One is that there is sufficient knowledge on the radiation sources which come from a nuclear weapon to assess the capability of a nuclear detection system by placing it in space and finding its response to the natural background.

This is one point of view. The other point of view is that ultimately one would want, after having set up a detection system, to fire——

Representative HOLIFIELD. Let me understand this first point. What you are saying is that there are enough neutrons and cosmic rays and so forth in space on a natural basis for you to test a detection instrument; is that right?

Dr. LEONARD. I think perhaps I will have to amplify that statement just a little bit.

The range of a given type of detector is broadly a function of two things; namely, the amount of radiation that comes from the weapon and the natural background in space. It is the same type of problem that has been discussed in detail here on the seismic detectors. That is, the question of getting the signal from the bomb high enough above the natural background to see it. One point of view would be that we have sufficient knowledge of the radiation that comes from the weapon to know that part of the problem without running a nuclear test. The remaining problem, therefore, would be to measure the radiation background in space in detail and find how the detection equipment responds to that background, and having done that we would then have an acceptable understanding of the capability of the detection system.

Representative HOLIFIELD. In other words, the information you get from the so-called Van Allen belt and other radiation elements in space would give you the capability of detecting the so-called background radiation and recording it, and then the objective of your machine would be to record variations from the background, on the assumption that those variations were manmade and, therefore, were nuclear tests?

Dr. LEONARD. Yes.

Representative HOLIFIELD. Do you know if there are any natural variations from background such as meteorite explosions and other types of explosions in space which might affect your detection instruments intermittently, and, therefore, introduce an element of confusion into your assumption that a variation was necessarily a manmade nuclear explosion?

Dr. LEONARD. This is certainly one of the basic problems that face us today.

Representative HOLIFIELD. We know on a seismograph we have background noise. We have had testimony that in order to avoid additions to the natural earth noise, such as that made by trucks or railroads or factories and that sort of thing, it is necessary to isolate the battery of seismological instruments.

It may well be that in outer space there are intermittent variations which would further complicate this matter.

Dr. LEONARD. This is certainly true. I think there is no question but what we are in much worse shape as far as the knowledge of the backgrounds that would confront this system as compared to the seismic instrumentation.

For one thing, we have never performed experiments to search for signals that might look like a bomb that could come from natural phenomena.

Representative HOLIFIELD. It is true that there are a great many solar explosions in space, meteorite explosions and that sort of thing.

Dr. LEONARD. I think from a theoretical standpoint, people who have looked at this in detail are optimistic about being able to separate out bomb signals from natural phenomena. On the other hand, this has never been looked at experimentally.

Representative DURHAM. Of course, it is a fact that you do not know much about the radiation background in that 150-million-mile orbit.

Dr. LEONARD. This is certainly true. We have only a meager amount of information.

Representative DURHAM. The whole thing is theoretical.

Dr. LEONARD. Largely.

Representative HOLIFIELD. Now could we get back to my question, assuming that we did want to make a calibrated shot, do you have any idea in mind what the time period would be before we could do it?

Dr. LEONARD. I do not think I can give a specific answer to that, but it is simply a matter of time to get ready for the test. The basic technology is available.

Representative HOLIFIELD. We will ask some of the later witnesses and see what their opinion is on this.

Thank you.

Dr. LEONARD. Thank you, sir.

(The full statement of Mr. Donovan follows:)

STATEMENT BY ALLEN F. DONOVAN

Gentlemen, I regret that due to a longstanding commitment I am unable to be present today to personally present my opinions on space vehicle requirements for systems to detect nuclear explosions in space. I have, however, prepared this written statement for your committee. In addition, I have arranged for one of my colleagues, Dr. B. P. Leonard, who has worked with me on studies of systems for the detection of nuclear explosions in space, to be present to answer questions and present such additional material as your committee may require. Should there be further interest in this general subject I would welcome the opportunity to appear before your committee at future hearings.

For your general information, my field of technical specialization lies primarily in the design and development of space vehicles, particularly those aspects related to propulsion, structures, guidance and control, and communications. I served as a member of Dr. Panofsky's delegation to Geneva with the assignment of providing information in the satellite and space vehicle area. By training I am an

aeronautical engineer, not a physicist, and my present position is vice president and director of the Advanced Systems Planning Division of Space Technology Laboratories, Inc., in Los Angeles, Calif. You probably know Space Technology Laboratories, or STL as it is usually called, as the organization responsible for the systems engineering and technical direction of the Air Force's Atlas, Titan, Thor and Minuteman ballistic missile programs. In the space field it was STL who built and launched the space probes Pioneer I, II, and V and the satellite Explorer VI for NASA under the management of the Air Force Ballistic Missile Division.

I have tried to anticipate the areas of interest to you in which I might supply information. I believe that there are primarily two. First, the ability to conduct nuclear tests in space and, second, the capability of our vehicles to install systems in space for the detection of nuclear explosions. Because of limitations of security on the capabilities of our space launching vehicles and related items, I will confine my remarks primarily to those items discussed at Geneva or which have previously been released.

First, let me discuss the ability to conduct nuclear tests in space. Perhaps, with the achievements of both the Soviet Union and ourselves in space flight, this topic does not need clarification. However, to be sure that there is no question about it, let me state simply that a vehicle such as the Atlas ICBM with appropriate upper-stage rockets could launch a nuclear device (together with sufficient instrumentation and radio equipment for a test) to a velocity above that required for escape from the earth. Radio communication with this test package and tracking of it in space could be accomplished as is being done with the space probe Pioneer V, which is currently relaying information to us from a distance in excess of 5 million miles. When the device had reached the desired distance, which could be in the range of tens of millions of miles, a radio command would separate the instrument package from the bomb. After a preset interval necessary to provide the desired separation, a timer would detonate the test device while the instrumentation recorded the pertinent data and radioed it back to earth. When space vehicles of the future with bigger payloads come into being, large weapons could be tested in space. Furthermore, part of the payload could be used to put a shield around the device to make it more difficult to detect the explosion. Shielding might also be accomplished by testing behind the moon, for example. The Soviets have already demonstrated with their Lunik III the ability to go behind the moon, and their demonstrated superior payload capability would give them an advantage over us conducting such tests at the present time.

Let me comment briefly on the safety problem. Many uninformed people have expressed concern over the possibility that the failure of a rocket vehicle during launch of a test device could result in a major catastrophe. I would like to say, merely, that it appears that the danger of such an accident producing a nuclear explosion in the atmosphere or on the earth can be adequately guarded against through techniques already developed. Therefore, let me summarize with the statement that testing in space is feasible even at distances up to tens of millions of miles and in such regions as behind the moon.

Let me next proceed to discuss satellite vehicle requirements for space detection systems. The principal systems discussed by the High Altitude Technical Working Group at Geneva were a far-earth satellite system, a solar satellite system, and an Argus satellite system. In addition, a near-earth satellite system was discussed as an alternate to the far-earth system if launching vehicle limitations made the far-earth system too difficult or expensive. The far-earth satellites were conceived as operating in orbits from 30,000 to 50,000 kilometers altitude or roughly 20,000 to 30,000 miles high. The system would consist of from four to six satellites arranged in orbits so that the detectors would survey all the space below them down to the surface of the earth's atmosphere and all space outward from their orbits except for the regions shielded from view by the moon, the sun, and other planets. To cover these regions and to assist in the detection of shielded explosions at large distances, the U.S. delegation proposed a system of solar satellites. The Soviet Union objected to adding these vehicles for detecting explosions behind the moon in spite of the fact the Soviet luniks demonstrated the capability of reaching this region. The Soviets took the position that the cost of launching solar satellites would be excessive and the communications problem too severe. Nevertheless, they were willing to accept the far-earth satellites in orbit at 50,000 kilometers. Strange as it may seem to those of you who have not been involved in the peculiarities of space vehicles, it takes a more powerful rocket system to put a satellite in orbit at 50,000 kilometers altitude than it does to launch one above the speed of escape into orbit around the sun. In either case, it requires a powerful multistage rocket system beginning

with an Atlas or larger size booster. Fortunately the development of space boosters by the Air Force and NASA will make launching vehicles available sooner than it appears that satellite payloads for detection can be developed. With respect to the satellite vehicle itself, suitable radio communication, tracking and control links, together with their space power supplies, have been developed and are being tested in Pioneer V. Devices for storing data and playing it back on command are in use.

The temperature control system for Pioneer V is functioning perfectly. Techniques for building lightweight satellite structures are available together with the means for properly stabilizing the satellites in orbit. Designs for instruments to detect the basic signals from a nuclear explosion have been proposed by Drs. Panofsky, Bethe, Latter and others. Development of these sensing instruments and the electronic analyzer which will discriminate between the signals from a nuclear explosion and the natural radiation in space must be accomplished. Then, detection instruments, analyzer, data storage, communication link, stabilizing system, temperature control system and surrounding structure must all be carefully integrated into a total satellite vehicle system matched to the launching vehicle. Flight trajectories must be worked out to place the satellite in the proper orbit while adhering to the requirements of range safety. Tracking and receiving stations must be established at appropriate points around the earth to command the satellite and receive its data. These must be suitably connected to a central command center and a data processing and analysis center must be provided to interpret the satellites output.

Fortunately, the Air Force is currently establishing under other programs a worldwide network of tracking stations of the type necessary to serve nuclear detection satellites. Thus, the technology and procedures for creating a nuclear detection ground system will exist in the near future. Work on other U.S. space programs sponsored either by the Air Force or the NASA is providing subsystems and technology as outlined previously for all aspects of the satellite vehicle except the basic nuclear signal detectors and the signal analyzer. Availability, therefore, of a first exploratory nuclear detection satellite is limited technically, first by the availability of the detectors and the associated analyzer and, secondly, by the time to integrate these units together with the storage system, communication system, power supply, temperature control system and stabilization system into a total satellite vehicle. This time is sufficient for procurement of launching vehicles. Launch-stand availability does not appear to pose a problem for the initial exploratory satellites. It will, of course, depend upon the number of other space launches planned and the relative priority assigned to the nuclear detection program.

Before closing, I would like to insert a word of caution so that the preceding statements on satellite vehicle development will not be misinterpreted. There are important questions as to the effectiveness of satellite-based systems for the detection of nuclear explosions at high altitude which can only finally be resolved by measurements of the natural radiation in space and testing detection system components in this radiation environment. I wish to make it clear that our present concept of a satellite detection system assumes certain things about this natural radiation. Only after this background is known and experience is gained from testing detection equipment in it can we accurately assess the effectiveness of any satellite system proposed. Hence, my statements on the ability to create and launch satellite vehicles of the presently conceived type while certainly encouraging, do not automatically insure that doing so will give us an adequate capability for detecting nuclear explosions in space. I am sure that Dr. Taschek and others will expand on this point in their testimony.

This concludes my prepared statement on the matters related to satellite vehicle requirements for nuclear detection systems. I would like to thank you for this opportunity to present my views on this subject and to again express my willingness to appear before your committee in the future.

Representative HOLIFIELD. Our next witness will be Dr. Richard Taschek, of the Los Alamos Scientific Laboratory, who will present testimony on the time scale for achieving nuclear detonation detectability. Dr. Taschek is currently conducting a program for the design and installation of detectors in satellites.

You are from Los Alamos. I want to apologize for holding you over extra time. You were here yesterday and saw how the hearings were going and the delay which seems unavoidable.

STATEMENT OF DR. RICHARD TASCHEK,[1] LOS ALAMOS SCIENTIFIC LABORATORY, LOS ALAMOS, N. MEX.

Dr. TASCHEK. That is quite all right. It is a pleasure to be able to report to you.

I did really have two portions to discuss which I will combine into a single one. One of them is the problem which has been discussed to some extent by Dr. Leonard; namely, the actual detection problem itself as it has presently been studied in a study already referred to made jointly by AEC, NASA, and ARDC (Air Research and Development Command).

I would like, therefore, to put into the record my comments on an approach to the problem of detection of nuclear detonations in free space. (See p. 363.) I would like to preface this by saying that I think it is clear from all the discussions that we have heard on the seismic problem that at any particular instant one does have a detection or surveillance capability in the high-altitude field as in the seismic field.

Of course, that capability may be zero, but there will be one which can be defined approximately, and I think one of the main functions of this meeting has been to try to define what at this particular instant is the surveillance capability.

This is one of the points to which I wish to address myself.

The identifying radiations from a nuclear detonation may be quite varied and large in number. In addition, the field or the medium in which the detonation takes place generally has an interaction with these radiations which can also be used for detection.

In the discussion, it will be assumed that the detonation takes place in field free space; namely, beyond about 10 earth radii and the detection methods under investigation here are based on earth satellites in orbits which are optimized for detection of detonations at distances which are normally beyond the trapped radiation belts.

A careful study of various detection methods and their applications under the conditions described leads one to the conviction that direct detection of the principal bomb radiations; namely, the X-rays, the gamma rays and neutrons, will give the best indication of a nuclear detonation.

This conviction is determined in part by the rather high degree of development of detector technology for these radiations and the high degree of predictability of the time history, intensity and spectral properties of X-rays, gamma rays, and neutrons emitted; namely, this is a question of predictability as Dr. Leonard already mentioned.

Other practical and well-understood methods of detection, such as the Argus effect and the fluorescence excitation of the earth's atmosphere, will not be discussed here primarily because their maximum range is more limited than for the methods described.

However, I would like to refer detailed questions on some of these other methods to Dr. Latter, if the committee should feel so inclined.

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[1] Born, Chicago, Ill., June 15, 1915.
Degrees: B.A., University of Wisconsin, 1936; M.S., University of Florida, 1938; Ph. D., University of Wisconsin, 1941.
Past Chairman of the Atomic Energy Commission's Nuclear Cross Section Advisory Group. Past Chairman of the Atomic Energy Commission's Tripartite Nuclear Cross Sections Committee. Present Chairman of the Atomic Energy Commission's European-American Nuclear Data Committee.
Fellow: American Physical Society, American Association for the Advancement of Science.
Staff member: Los Alamos Scientific Laboratory, 1943 to present.

At any instant the extreme detection range with a particular detector is likely to be determined by interfering natural backgrounds of one or more kinds. These may be steady, slowly varying, or, worst of all for our problem, pulsed with a signal duration similar to that expected from nuclear detonations.

Such expected interference implies early studies of background at the expected orbital distances with a priority determined largely by the properties of the prototype detection devices to be used in later stages of such a program.

The existence of the regions of high intensity trapped particles surrounding the earth places a criterion on the orbits at which the satellites may be flown; namely, the measured intensities already observed force the detection satellites outside the trapped radiation zone beyond about 30,000 nautical miles.

The problems which are connected with electronic logical decision making elements which have to be aboard the satellite are very complicated indeed, because of the requirements which are placed on them by the information coming out of the detectors. The complication of this part of the information chain is so great that considerable effort must be expended to design and construct the logic elements with a preassigned reliability which is commensurate with the data requirements.

I am saying here that it is not even possible to go in and repair equipment which has failed as in seismic stations, but you have to know ahead of time that nothing will happen to the equipment for some particular length of time.

APPROACH TO THE PROTOTYPE DETECTOR SYSTEM

An overall approach considered to be the most expeditious for solution of the problem at hand on a time scale which may be considered as one of reasonable deliberation is the following:

First, there should be a three-pronged, approximately parallel attack on some preliminaries considered a necessary preparation for the full-scale experimental satellites. Thus, it is considered highly desirable to have, in addition to low and high altitude background measurements, low and high altitude probe and piggyback flights intended to test instrumentation problems on the one hand, and preprototype development plus actual measurements of their responses to background radiation on the other.

Most of the necessary parameters leading to success in the first experimental satellite should have been given an initial fix during these early flights.

Following these preliminaries, it is believed that after placing three experimental satellites in orbit, the requirements for a successful prototype surveillance system should have been met.

I would like to emphasize that this is not an operational system but a prototype system.

The detectors used would be designed around predictable nuclear radiations and known technologies for sensing these radiations.

Provision would be made to uniquely identify nuclear detonations by the time history of the signals on one or more detection schemes with a negligible false alarm rate.

Provision would be made for advances in detection science and technology. A criterion which is used in the Geneva report is a false

alarm rate of one per hundred years. This does not mean a spoofed false alarm rate, but one which is due to natural background about which we can do very little, or nothing.

Representative BATES. Is this the prototype you expect to take 2 years to develop and have ready for launching?

Dr. TASCHEK. This is the prototype, the first satellite we expect we could have ready in a period of 2 years.

Now for the conceptual surveillance satellite:

In order to establish the contents of the first experimental satellite, we will find it useful to discuss a conceptual surveillance satellite first, in order to determine a frame of reference within which to work. In the following discussion, the reference bomb is a nominal, unshielded 10 kiloton device.

At high specific yields the X-ray signal is emitted in $5\text{–}10 \times 10^{-8}$ seconds.

At low specific yields the X-ray signal is emitted in about 10^{-6} of a second or longer.

Prompt gamma rays are emitted from a few $\times 10^{-8}$ seconds to 10^{-6} seconds.

Delayed gamma rays appear in $\sim$1 second.

Neutrons are spread out as long as 2,000 seconds depending on distance, since at 1 Mev. they travel 14,000 kilometers per second.

I might point out that neutrons are considered to be of interest out to about 2,000 seconds of observation. Note a one-million volt neutron travels only 14,000 kilometers per second, which means that the flight time is of considerable importance.

The neutron detector is the first one which I will discuss. The simplest device is the neutron detector consisting of either BF_3 counters or Li I scintillation crystals immersed in a plastic neutron moderator. A nuclear event is recognized by the fact that the neutron background count is exceeded by a predetermined amount and that one or both the other detection methods to be described have triggered; from this event on, the arrival time of neutrons is measured in logarithmic time intervals from $\sim$1 second to $\sim$2,000 seconds, giving both a total yield and distance from the time history.

Under "no alarm" conditions, background is continuously monitored and transmitted; under alarm conditions, determined either by a photon trigger or by intensity exceeding a threshold, the logarithmic time scan goes into operation through a storage unit.

Representative HOLIFIELD. I am going to ask you the same question as Dr. Leonard. Are there phenomena in space which might trigger this alarm system?

Dr. TASCHEK. We do not know for sure, but in the case of neutrons we suspect very strongly that it will be one of the safest from background radiations. A reason for this is that neutrons coming from the sun take a long time to get here. A neutron decays into a proton and electron with a half life of about 10 minutes. Therefore, low energy neutrons are very unlikely to ever reach the earth.

There will, however, be neutrons made in the satellite itself by energetic primary cosmic rays. This will be one of the things which will have to be determined in early flights of both probes and of the initial satellite itself; namely, the background rate.

Representative HOLIFIELD. Are neutrons emitted by a meteorite explosion somewhere between the sun and the earth?

Dr. TASCHEK. I think this is exceedingly unlikely.

Representative HOLIFIELD. Is there any other phenomenon which takes place which causes an outflow of neutrons?

Dr. TASCHEK. It is quite likely that neutrons are born on the sun and that some of them do get out. In order to see them, they would have to be very, very energetic particles and we do not expect to see primary neutrons.

If we were down deeper in the atmosphere we would see secondary neutrons and, indeed, these are seen.

Representative HOLIFIELD. Could you not see primary neutrons from the sun just the same as from a nuclear device?

Dr. TASCHEK. It is unlikely, because they die before they get to the earth unless they have almost billion-volt energy.

Representative HOLIFIELD. I am not talking about getting to earth.

Dr. TASCHEK. Excuse me; by earth I meant a satellite about 100,000 miles or so from the earth. In fact, there are more neutrons at 50,000 feet than there are likely to be at 100,000 miles. I might point out here that neutrons are possibly the most predictable of all the radiations and also of the effect that we will get from background.

To continue, although the range is limited to about 3×10^5 kilometers, approximately moon distances, anticipated backgrounds are small, the device is simple and could indeed be put into the first satellite in an essentially "ready" condition.

Gamma rays: A conceptual gamma ray detection system is made up of 6 100 cm^2 area plastic scintillators, each mounted on a 2-inch photomultiplier and shielded by Pb and Be against bremsstrahlung [1] and protons with energies greater than $\sim$25 Mev.

The criterion for a nuclear event would be the occurrence of a gamma ray coincidence within a time interval of $\sim3\times10^{-8}$ seconds. An additional requirement would be that seven Geiger counters distributed throughout the satellite not be discharged coincidentally since this could arise from a local cosmic ray particle shower.

The detection range with a satisfactory false alarm rate is approximately 3×10^6 kilometers, roughly 3 million kilometers. With a higher false alarm rate, but corroborating signals, appreciably greater ranges are possible.

This is a factor of 10 more than what we feel the neutron detection capability might be as expressed in terms of range for a 10-kiloton bomb. It is about 300,000 kilometers for the neutron detector and about 3 million kilometers for the gamma ray detector.

I will now discuss X-ray detectors.

Great emphasis is placed on the use of X-rays as probably the best detectable radiation since as much as 50 percent of the total energy of a nuclear detonation is free space appears as high temperature radiation, that is, soft X-rays.

A conceptual X-ray detector unit for initial consideration is a thin—16 mil—plastic phosphor covered with Be windows on a 2-inch photomultiplier tube. Four such detectors form a "clutch" and eight clutches are used to cover the total solid angle available.

The simplified criteria to be met by an "alarm" signal would be: (*a*) a fixed threshold signal level must be exceeded in at least three out of the four counters in a clutch; and (*b*) the decay time of the signal must have a shape appropriate to a bomb signal.

[1] A German word meaning literally "braking radiation," denoting the process of producing an electro magnetic radiation (or the radiation itself) by the acceleration of a fast charged particle, such as an electron, undergoes when it is deflected by another charged particle, such as a nucleus.

I think you recognize here a simplicity in some senses over the seismic detection in that we do know, as Dr. Leonard has already pointed out, a great deal about the shape, the time dependence or history or "signature" of a signal coming out of a bomb.

The detection range of Scintex under the criteria already set forth is about 3×10^8 kilometers, that is, roughly solar orbital distances. This device is, however, particuarly sensitive to the interposition of a shield around the bomb which reduces not only the X-ray yield, but more unfavorably, reduces the temperature and, therefore, the spectrum of the radiation below the sensitive region of the fluor with its Be windows.

To overcome such shielding countermeasure effects, Dr. Panofsky originally proposed the use of an open photomultiplier system which would be sensitive to X-radiation of much lower energies. Under these conditions even shielded nuclear detonations could be detected at extreme ranges.

Those ranges will, however, be dependent upon the precise action of the shield which in turn will depend upon the design of the shield.

Detectors of this kind under the name of Panex are being designed and tested, but because of their great sensitivity to many kinds of radiation, including low energy particles, they require much more study of their properties than the other types discussed.

Recently there has been a great deal of development work in the area of solid state detectors and these are being pursued very actively and show considerable promise.

THE FULL-SCALE EXPERIMENTAL SATELLITE

In line with a deliberate approach to a final operational surveillance system, carefully thought out prototype satellites have been considered. Thus, a possible first satellite payload contains devices which are considered to be a first step in that direction; namely, portions of the above discussed conceptual detection schemes would be flown under test conditions in which further detailed information would be provided by so-called support experiments.

In particular, these support experimental devices would be time correlated with the prototype detection units and with other phenomena; for example, solar events in order to determine the preprototype response characteristics to the details of background events.

I think this is the question that the chairman asked about the effect of events in space which vary in time. These are known to occur, but we do not really have enough information on them.

This kind of information would then allow both operating characteristics to be changed—for example, by command—or design changes to be made in later models.

In addition, information would become available on such parameters as the fraction of time that a particular range capability existed averaged over the lifetime of the system.

Later satellites would progress in the direction of less support experimentation and the extension of Gamex and Scintex toward the full systems. One would hope, therefore, to progress toward greater simplicity and more reliability in later satellites than in the first.

It is worth while mentioning here that the first satellite would have a logical decision system which would either transmit the ambient

background data through a commutated, but otherwise continuous, data transmission system or, when an appropriate alarm signal appeared, switch to an alarm channel which would store the alarm data in a memory for later transmission as many times as interrogated.

A logical decision system is made up of many components which collectively allows prediction of a mean lifetime for the detection system. It would probably be necessary that the design be so adapted that there be a 90-percent probability of obtaining a 3-month lifetime for data reception.

The data from the detectors would be processed in the logics and then transmitted to the ground in digital form. On the ground, the data would be recorded on tape and sent to a central data handling laboratory for reduction. In some cases, particularly early in lifetime of the satellite, the data would be used to provide new commands for detectors or other elements of the satellite.

Below, I have, according to your instructions, shown a schedule and costs under a set of conditions which I felt were the ones indicated in the agenda. I think that these will in part at least agree with comments which I believe you will get from Mr. Beyer.

The schedule below shows approximately how the payload costs for a program leading to three successful satellites might be distributed exclusive of any launch rocket, ground communication or systems engineering charges. It is presently believed that under the most pessimistic assumptions 11 payloads would have to be fabricated for 3 successful launchings.

Fiscal year	Number of payloads	Probe and piggyback experiment	Detector development and construction	Payload logics power and telemetry	Data reduction	Total
		Millions	*Millions*	*Millions*	*Millions*	*Millions*
1961	1	$0.4	$1.0	$2.2		$3.6
1962	6	.4	1.0	7.4		8.8
1963	4	.4	1.0	5.4	$0.6	7.4
1964			1.0	1.4	1.2	3.6
1965			.5	.5	1.0	2.0

I would like merely to point out that what is discussed in the table is restricted to the satellite itself and has nothing to do with the problem of launching the satellite or with the ground communication system.

Representative HOLIFIELD. Let me understand you. The satellite itself is considered here and not the propulsion vehicle?

Dr. TASCHEK. Not the propulsion vehicle.

Representative HOLIFIELD. Nor the ground crew, nor the telemetering communication system?

Dr. TASCHEK. The telemetering in the satellite is considered but not the ground installation, namely, the reception of the telemetered signal.

Representative HOLIFIELD. Which would have to be several places on the earth's surface.

Dr. TASCHEK. This is a worldwide net. I think under time scale I have just mentioned briefly that it is understood that the presently established worldwide net for space communications can be used with essentially minor additions.

Representative HOLIFIELD. I was going to ask you if the present satellites that are being launched that are sending back information to the earth are obtaining any of this data that you refer to here?

Dr. TASCHEK. The most important piece of data which might have been gotten from Pioneer V unfortunately was lodged in an experiment which got thrown off of Pioneer V at the last moment. In the sixth satellite which Dr. Leonard discussed, there was a great deal of data obtained on the compositions in energy and number of the great radiation belts.

In fact, it was as a result of these data that the final orbits for the far-earth satellites were chosen to be out almost twice as far as we had originally assumed might be necessary.

Representative HOLIFIELD. Do you have plans at this time for any devices in satellites which are now being planned to obtain any of this information?

Dr. TASCHEK. Yes. We have underway discussions with NASA and we have planned vehicle flights piggyback on Atlas missiles, and we have flights planned which are to go just to the outer edge of the radiation belt in probes of some future NASA and ARDC flights.

Representative HOLIFIELD. So this information will be coming in, you might say, automatically but not on a priority basis

Dr. TASCHEK. It will not be coming in automatically because normally the information that we require is not very exciting to the people who are doing pure, undirected research.

Representative HOLIFIELD. How much money is involved in your study?

Dr. TASCHEK. In the part of the studies which have to do with what we were discussing a moment ago, namely, these preliminary measurements, I think you will see under probe and piggyback experiments a column which lists four-tenths of a million dollars per year in 1961, 1962, and 1963.

Representative HOLIFIELD. Does that mean that it is already started or it is planned to be started?

Dr. TASCHEK. This program has started on a catch-as-catch-can basis. It is being done by diversion.

Representative HOLIFIELD. Part-time work of different individuals?

Dr. TASCHEK. Part-time work and by getting onto other people's rides, and so on.

Representative HOLIFIELD. You are hitchhiking at the present time?

Dr. TASCHEK. Mostly we are hitchhiking; that is right.

Representative HOLIFIELD. How many people are working in your group?

Dr. TASCHEK. At Los Alamos Laboratory there are presently about 25 staff, with otherwise normal laboratory support.

Representative HOLIFIELD. Is this full-time or part-time?

Dr. TASCHEK. This is full-time. At the Sandia Laboratory which is concerned with the problem of developing the logics, a very difficult problem indeed—this is the electronic decision making elements—I believe they presently have about 30 people also. If we proceed in this direction they will have to increase their effort in the area.

Representative HOLIFIELD. Mr. Durham?

Representative DURHAM. Doctor, what you are really talking about here is 1962, the one that will carry a payload of 6⅒ and will cost $8.8 million. (See table, p. 355)

Dr. TASCHEK. Excuse me. I was referring to column 3 labeled "Probe and Piggyback Experiments."

Representative DURHAM. I say, really the one you are describing here and the one that would pay off in all of these far distances would be the fiscal year 1962?

Dr. TASCHEK. Yes. The satellites certainly begin to be the major expenditure in 1962.

Representative DURHAM. I notice you have a lifetime expectancy on that of 3 months for its use.

Dr. TASCHEK. That is about all we expect from it.

Representative DURHAM. That means a fourth of a year, and in 4 years it would amount to about $36 million.

Dr. TASCHEK. In this program, as it has been laid out in the triple working group which just finished its study, it was assumed that the answer would have been obtained to the question of how do you set up a surveillance system after three successful experimental satellites.

In order to get those three experimental satellites, it is likely that under the worst conditions we would have to launch nine.

Representative DURHAM. Do you have any direct appropriations for the program you are carrying on at the present time at all?

Dr. TASCHEK. There is none.

Representative DURHAM. Has anybody requested any?

Dr. TASCHEK. I believe we have requested support through the Atomic Energy Commission. Otherwise the program, as a unified program, has only been established through this triple analysis providing a development and funding plan. I believe that Mr. Beyer will speak to that.

Representative DURHAM. I gather from your statement that it seems to be pretty well scattered around in different directions.

Dr. TASCHEK. This is certainly true.

Representative DURHAM. What is your opinion? Would you feel better if it was consolidated in some form with direct appropriations for specific projects?

Dr. TASCHEK. The problem, in my opinion, falls into three fairly nicely defined areas. One of them is the detection devices, plus their associated electronics. We might say that the satellite which finally flies is a unit that is separable.

On the other hand, another easily defined category is the rocket vehicle required to launch that unit.

Representative DURHAM. Of course, this is a complicated field. We all know that, although it is a very important one. I have said that many times in regard to the political propaganda going all around the world. If you do not care to answer this do not do it.

How do you feel today we compare with the Russian system in this field? If you do not care to answer, you do not have to.

Dr. TASCHEK. You mean in this detection business?

Representative DURHAM. Yes.

Dr. TASCHEK. From satellites?

Representative DURHAM. From satellites.

Dr. TASCHEK. My feeling, not backed by very much real information, is that we probably lag quite seriously in the capability, for launching satellites.

On the other hand, I think if we can get a satellite launched that we can probably make a much better one than they can.

Representative DURHAM. That would be 2 years from now, at the best?

Dr. TASCHEK. Might I discuss some questions on the time because this might answer Mr. Durham's question?

Representative HOLIFIELD. Very well; you may proceed.

Dr. TASCHEK. The problem on the time scale, I believe, is the following. The program which has "due deliberation," with quotation marks written about it, and is covered in the development and funding plan, will certainly require 2 years to reach a point of first satellite launch. On the other hand, we do have some kind of a limited capability at any instant in the seismic business and it probably would make some sense to have some limited capability, and we must understand that it is limited, for detection of nuclear detonations in space.

It is possible, I feel, to provide such limited capability in several ways. I might state that one of them is a ground-based one, which has to do with detection of fluorescence radiation from the atmosphere.

I think you heard this mentioned by Dr. Graves yesterday. It has a potential range at this same tonnage that I mentioned of something like moon distances. It will be smaller or larger, but this will depend finally on further research.

Representative HOSMER. Referring to what?

Dr. TASCHEK. Of, say, moon distances. A range of up to 250,000 miles from the surface of the earth.

Representative BATES. Doctor, you indicated in response to a question of Mr. Durham, of our shortcomings relative to Russia in respect to launching. I presume you meant thrust and propulsion.

Dr. TASCHEK. Yes.

Representative BATES. What is the weight of one of these which would indicate we are short in this particular field once you devise a vehicle? Is that a real heavy job?

Dr. TASCHEK. The devices which we have discussed for a large number of technical reasons probably require that the Atlas Agena-B rocket-launching devices be used in order to put a total payload of, let me say crudely, 400 pounds into orbit.

Representative BATES. Then there is no shortcoming.

Dr. TASCHEK. We do not have a shortcoming under these conditions.

Representative BATES. And doing the job which you want to do.

Dr. TASCHEK. That is right.

Representative BATES. The answer to the question was outside of the orbit of our particular concern now.

Dr. TASCHEK. I think so.

Representative BATES. Then you are talking about a 2-year period in the future, which is quite another matter, too.

Dr. TASCHEK. That is right.

Representative BATES. How many of these satellites would you expect to eventually have in orbit once you have this program underway?

Dr. TASCHEK. You are discussing now an operational system?

Representative BATES. Yes.

Dr. TASCHEK. I think an operational system is felt to be satisfied if we restrict it to the far-earth orbits with six satellites simultaneously in orbits.

Representative BATES. This is 24 a year.

Dr. TASCHEK. This is likely to be a 24 a year, provided we cannot improve reliability. It may be that reliability can be improved. In fact, this is one of the major points where one would like to have more weight available.

Representative BATES. What you you expect the cost at that time to be?

Dr. TASCHEK. The cost of the operational system, I am sorry, I am not really capable of discussing. I think this has been guessed at in Dr. Panofsky's report and elsewhere. The operational system so far as I know has never been very carefully studied.

Representative BATES. By operational you mean just pertaining to the satellite, but not the launching and not all the ground crew?

Dr. TASCHEK. No; excuse me. By "operational" I mean that system which we are after, after some period of time in the business; namely of having a 24-hour-a-day, every-day-of-the-year set of six satellites at far-earth orbits which will, under present conditions, have been replenished, as you have said, probably at the rate of a total of 24 per year.

Then you will have to investigate the questions of how many rocket flights you will require in order to launch them on the basis of one out of three, one out of two. How many reserve satellites you have; how fast you can keep this system going. We then get into the problem which was discussed a little bit yesterday on the seismic program by the last gentleman who talked.

Representative HOLIFIELD. If the gentleman will yield on that point, you spoke of the missiles necessary to keep 24 of these objects in orbit. It is commonly accepted that the present cost of an Atlas missile is about $1 million.

Dr. TASCHEK. I am sorry; I am really incompetent in this area. I am sure Dr. Leonard could give you rather precise figures.

Representative HOLIFIELD. The other question I want to ask you in this: You spoke of a 3-month duration. Is it not true that the SNAP device is almost ready, which would give much longer power source in a satellite?

Dr. TASCHEK. This is quite true. I am sure that power will not be the failing that we will have.

Representative Holifield. There is such a discrepancy between the capability of the SNAP nuclear power system and the 3 months which I assume is based on battery power.

Dr. TASCHEK. No; I am sorry.

Representative HOLIFIELD. Or solar power.

Dr. TASCHEK. This is one of the things which will eventually deteriorate. The problem of reliability which has been very carefully looked at has its major factor in the some 50,000 logical decision elements which are mostly solid-state devices.

Representative HOLIFIELD. I see. So your reliability is really figured on technical apparatus outside of the power?

Dr. TASCHEK. On the normal breakdown of resistors, transistors, capacitors, and devices of this kind.

Representative BATES. What efficiency would you hope to obtain once this system is in operation?

Dr. TASCHEK. How do you mean?

Representative BATES. What is the capability of the system in detecting an explosion once you have it in operation?

Dr. TASCHEK. The capability of these devices, I think, I have indicated in terms of range. I believe it has been mentioned and emphasized that at extreme range, which is like the earth's orbit around the sun, that the only sure-fire detector is very likely to be the open X-ray detector.

This will depend on whether we are able to in part learn enough about background and in part do something about it to make sure that that open X-ray detector can be used. There is no problem about detection of an unshielded bomb. Almost anyone who tries to beat the system and if he goes to as much trouble as going to the other side of the earth's orbit will also have gone to the trouble of doing some shielding, because he will be very interested, indeed, in exploding that device in order to learn something.

So I think that from earth's orbits, with the 6-satellite detection scheme, with open photomultipliers which are not now developed but are in the development stage, that the chances are really rather good that one will detect nuclear detonations, say, above 100 kilotons or so.

Representative BATES. What do you mean by "rather good"—50, 75, 95 percent, or what?

Dr. TASCHEK. This is even much harder to predict than for the seismic system. In fact, this depends entirely on what your own prebelief happens to be. I do not know the answer to that.

Let me say it the other way around. I think for an unshielded nuclear detonation and for the devices that we have discussed, where you do not require the open photomultipliers, but those we know how to build today, that the chances are probably about 75 percent, with a full detection satellite system that one will detect and recognize a nuclear detonation in the 20 kiloton range.

Representative BATES. Unshielded?

Dr. TASCHEK. Unshielded.

Representative BATES. I notice in your conclusions which you have not quite reached, you say that you believe that the approach to this detection problem is a conservative one with a high probability of success?

Dr. TASCHEK. That is right.

Representative BATES. Is that unshielded?

Dr. TASCHEK. Unshielded.

Representative BATES. So we get right back to the same point that we did on the underground with decoupling.

Dr. TASCHEK. Precisely. There is always a countermeasure for every measure.

Representative HOSMER. Doctor, I would like to ask you in respect to this time scale and cost estimate that you have made here going through June 30, 1965, is that on the assumption that you and the Livermore group are working together?

Dr. TASCHEK. This is really not on the assumption that anybody is working, but is based on the development and funding program which is a result of this joint program that has been carried out.

Representative HOSMER. As a result of the joint program?

Dr. TASCHEK. As a result of the joint program that we have been able to come up with these figures.

Representative HOSMER. What if your group at Los Alamos worked along these lines and the group at Livermore took the position that the cost of launching solar satellites would be excessive and the communication problem too severe——

Dr. TASCHEK. If we had taken this position?

Representative HOSMER. If Livermore had taken that attitude and you had retained the attitude that you have, how much progress do you think you would have made, or could you have made any?

Dr. TASCHEK. I think the laboratories probably could still work independently on taking their own particular approach.

Representative BATES. Then assuming you worked independently and each arrived at some type of conclusions, would you believe that at that point you could get together on anything?

Dr. TASCHEK. I should certainly think on any technical conclusions one should be able to do so.

Representative BATES. Even if Livermore for some reason other than scientific did not want to agree with you?

Dr. TASCHEK. If it is a nontechnical reason it is a matter of opinion.

Representative BATES. They could stall it forever, in other words.

Dr. TASCHEK. Yes. Although I think there is a very interesting point here, that our assumption at Los Alamos has been how do you build detection devices? I think the really major contribution which came from Livermore some time ago was to point out that shields could be built, and then they made this very careful machine calculation—in fact, a set of machine calculations—which indicated the rather precise effectiveness of shields.

Here again I think one can point out that even those people who are in the detection business must also consider the countermeasures not merely qualitatively but as if they themselves were going to use the countermeasures. It is in that great detail that the problem must be studied.

Representative HOSMER. This time scale and so forth, I think you have qualified it by saying that at the middle of 1965 you would not have an operational surveillance capacity.

Dr. TASCHEK. That is correct.

Representative HOSMER. But all this goes to the foundation for an operational system.

Dr. TASCHEK. This was the approach.

Representative HOSMER. Dr. Taschek, you were requested to come here and testify about this particular subject?

Dr. TASCHEK. Yes.

Representative HOSMER. Did anybody tell you to put a pitch on it one way or the other?

Dr. TASCHEK. Let me see. I think I came back from Washington Friday morning, and I had a letter instructing me to appear with 75 copies of this thing on Tuesday, which meant that I had to leave Monday. So I did not talk with anybody except the secretary while I was writing this.

Representative HOSMER. It is your own idea?

Dr. TASCHEK. This?

Representative HOSMER. Yes.

Dr. TASCHEK. This is an abstraction—I thought I made this clear—and condensation of the material which is contained in the development and funding plan which has been submitted to ARPA by the three agencies under this tripartite action.

Representative HOSMER. Did anybody in Government or out ask you to make detection look to the committee more difficult than it actually is?

Dr. TASCHEK. No. Absolutely not.

Representative HOSMER. This is your honest answer?

Dr. TASCHEK. Yes.

Representative HOLIFIELD. In other words, as well as presenting this summary and abstract of your study and your project planning, this is also your professional opinion as to the merit of the substance of it?

Dr. TASCHEK. Yes.

Representative HOLIFIELD. Mr. Ramey?

Mr. RAMEY. Will your far-earth system detect tests behind the moon?

Dr. TASCHEK. The far-earth system as presently conceived with the detectors that are in there is likely to have considerable difficulty making detection behind the moon.

Mr. RAMEY. How can you detect such shots?

Dr. TASCHEK. I believe you will remember that Dr. Panofsky pointed out that the fission fragments from a bomb exploded behind the moon will eventually expand out so that they appear around the edge of the moon. I think under the conditions that he mentioned, where those fission fragments were unimpeded, then the detection of gamma rays and even delayed neutrons would be possible with the present detection satellites.

I think one has to take into account very carefully, however, as to another countermeasure which could be the impeding of the flight of the fission fragments. Remember that that device that Dr. Panofsky mentioned will depend on the so-called delayed emission of gamma rays and neutrons.

That dies off very rapidly with a half life—a variable half life—the longest half lives are no longer than a few seconds. The very longest are 55 seconds. Therefore, if the fission fragments are impeded long enough, and I am not sure this can be done, although I am sure it can be done in part by shielding, then the device which we will put up will not work.

It will not have sufficient sensitivity. A quantitative analysis of that is very difficult. We have not made that. We are merely aware of the fact that one can do the detection

Representative HOLIFIELD. Dr. Taschek, we have you programed for further testimony at the last part of the session on the high altitude detection techniques. Have you or have you not covered that, or do you have an additional presentation at that time?

Dr. TASCHEK. No; I believe I have now covered that. I believe I have also covered the question of time scale.

Representative HOSMER. May I ask one other question, Mr. Chairman, in connection with that time scale?

Representative HOLIFIELD. Yes.

Representative HOSMER. Say if by December 31 of this year you had to sit down and write up specifications and so forth, including the types and nature and functions and design of the instrumentation for a system which would have 75-percent chance of detecting a nuclear explosion in space, how much of that could on December 31 be definite? How much of it would have to be specified only in a general way?

Dr. TASCHEK. If you do not mind my adding another qualification, and that is that the detection capability refer to a distance up to moon orbit or possibly somewhat beyond.

Representative HOSMER. To be realistic, this must detect any explosion at any point within the capability of being made. You are not going to wind up at 225,000 miles only?

Dr. TASCHEK. I think it has been pointed out there that each device has its own limitations. What I was going to say was that for this shorter range, it would be possible to come up immediately with, I suspect, approximately the probability that you mention, a detection system.

Representative HOLIFIELD. What probability?

Dr. TASCHEK. This was a 75-percent probability. On the other hand, to make this same statement about sun-earth orbit distance, which seems to be the other distance that we talk about, that probability, I suspect, is very low.

Representative HOSMER. In other words, you could just draw up the broad outline of what perhaps it should contain.

Dr. TASCHEK. I think it would be much better than that. In fact, I think we could draw up exactly what it should contain including the logical decision system, because there has been an appreciable amount of work.

The thing that we could not do is answer your question about the 75 percent because we do not have enough information on background. We do not have enough information on the effect of background on our instruments which we would have to throw up there with no prior information.

Representative HOSMER. So the design of instruments and all those things have to be left for later decision and agreement among whoever is going to do the job?

Dr. TASCHEK. No. I think our approach would be that you design the instruments now. You put them up there and you find out what happens to them. That will then tell you what the range is. It will also tell you what the probability is.

Representative HOSMER. But if you were frozen to those instruments by some kind of an agreement, you would not be able to make the improvements that were indicated?

Dr. TASCHEK. Then we would be in trouble.

Representative HOSMER. Thank you.

Representative HOLIFIELD. Thank you very much, Dr. Taschek, for your presentation, and particularly for staying over an extra day. We know it has been of some inconvenience to you and the committee appreciates it.

Dr. TASCHEK. Thank you.

(Dr. Taschek's prepared statements follow:)

AN APPROACH TO THE PROBLEM OF DETECTION OF NUCLEAR DETONATIONS IN FREE SPACE

(By Dr. Richard F. Taschek)

The identifying radiations from a nuclear detonation may be quite varied and large in number. In addition, the field or medium in which the detonation takes place generally has an interaction with these radiations which can also be used for detection.

In the discussion which follows it will be assumed that the detonation takes place in field-free space, namely, beyond about 10 earth radii, and that the detection methods under investigation are based on earth satellites in orbits optimized for detection of detonations at distances beyond the trapped radiation belts.

A careful study of various detection methods and their application under the conditions described leads one to the conviction that direct detection of the principal bomb radiations namely, X-rays, gamma rays, and neutrons will give the best indication of a detonation. This conviction is determined in part by the rather high degree of development of detector technology for these radiations and the high degree of predictability of the time history, intensity, and spectral properties of X-rays, gamma rays, and neutrons emitted. Other practical and well-understood methods of detection such as the Argus effect and fluorescence excitation of the earth's atmosphere will not be discussed here, primarily because their maximum range is more limited than for the methods described.

GENERAL PROBLEMS AND DIFFICULTIES

At any instant the extreme detection range with a particular detector is likely to be determined by interfering natural background of one or more kinds. These backgrounds may be steady, slowly varying or, worst of all, pulsed with a signal duration similar to that expected from nuclear detonations. Such expected interference implies early studies of background at the expected orbital distances with a priority determined largely by the properties of the prototype detection devices to be used in later stages of the program.

The existence of the regions of high intensity trapped particles surrounding the earth places a criterion on the orbits at which the satellites be flown; namely, the measured intensities force the detection satellites outside the trapped radiation zone beyond about 30,000 nautical miles.

The problems connected with the electronic logical decision-making elements on board the satellite are very complicated because of the requirements placed on them by the information coming out of the detectors. The complication of this part of the information chain is such that considerable effort must be expended to design and construct the logic elements with a preassigned reliability commensurate with the data requirements.

APPROACH TO THE PROTOTYPE DETECTOR SYSTEM

An overall approach considered to be the most expeditious for solution of the problem at hand on a time scale which may be considered as one of reasonable deliberation is the following:

First, there should be a three-pronged, approximately parallel attack on the preliminaries considered a necessary preparation for the full-scale experimental satellites. Thus, it is considered highly desirable to have, in addition to the low and high altitude background measurements, low and high altitude probe and piggyback flights intended to test instrumentation problems on the one hand, and preprototype development plus actual measurements of their responses to background radiation on the other. Most of the necessary parameters leading to success in the first experimental satellite should have been given an initial fix during these early flights.

Following these preliminaries it is believed that after placing three experimental satellite in orbit, the requirements for a successful prototype surveillance system should have been met.

The detectors used would be designed around predictable nuclear radiations and known technologies for sensing these radiations.

Provision would be made to uniquely identify nuclear detonations by the time history of the signals on one or more detection schemes with a negligible false alarm rate.

Provision would be made for advances in detection science and technology.

CONCEPTUAL SURVEILLANCE SATELLITE

In order to establish the contents of the first experimental satellite we will find it useful to discuss a "Conceptual Surveillance Satellite" first, in order to determine a frame of reference within which to work. In the following discussion the reference bomb is a nominal unshielded 10 KT device.

At high specific yields the X-ray signal is emitted in $5\text{–}10 \times 10^{-8}$ sec

At low specific yields the X-ray signal is emitted in $\gtrsim 10^{-6}$ sec

Prompt γ-rays are emitted from a few $\times 10^{-8}$ sec to 10^{-6} sec

Delayed γ-rays appear in ~ 1 sec

Neutrons are spread out as long as 2,000 sec depending on distance since at 1 Mev they travel 14,000 km/sec.

Neutrons

The simplest device is the neutron detector consisting of either BF_3 counters or Li I scintillation crystals immersed in a plastic neutron moderator. A nuclear event is recognized by the fact that the neutron background count is exceeded by a predetermined amount and that one or both the other detection methods to be described have triggered; from this event on, the arrival time of neutrons is measured in logarithmic time intervals from $\sim$1 sec to $\sim$2,000 sec. giving both a total yield and distance from the time history. Under "no alarm" conditions background is continuously monitored and transmitted; under alarm conditions, determined either by a photon trigger or by intensity exceeding a threshold, the logarithmic time scan goes into operation through a storage unit.

Although the range is limited to about 3×10^5 km, approximately moon distances, anticipated backgrounds are small, the device is simple and could indeed be put into the first satellite in an essentially "ready" condition.

Gamma rays

A conceptual γ-ray detection system is made up of six 100 cm^2 area plastic scintillators each mounted on a 2-inch photomultiplier and shielded by Pb and Be against bremsstrahlung and protons with energies greater than $\sim$25 Mev.

The criterion for a nuclear event would be the occurrence of a γ-ray coincidence within a time interval of $\sim 3 \times 10^{-8}$ sec. An additional requirement would be that seven Geiger counters distributed throughout the satellite not be discharged coincidentally since this could arise from a local cosmic-ray particle shower.

The detection range with a satisfactory false alarm rate is approximately 3×10^6 km. With a higher false alarm rate, but corroborating signals appreciably greater ranges are possible.

X-rays

Great emphasis is placed on the use of X-rays as probably the best detectable radiation since as much as 50 percent of the total energy of a nuclear detonation in free space appears as high temperature radiation, i.e., soft X-rays.

The conceptual X-ray detector unit for initial consideration is a thin (16 mil) plastic phosphor covered with Be windows on a 2-inch photomultiplier tube. Four such detectors form a "clutch" and eight clutches are used to cover the total solid angle available.

The simplified criteria to be met by an "alarm" signal would be: (a) a fixed threshold signal level must be exceeded in at least three out of the four counters in a clutch and (b) the decaytime of the signal must have a shape appropriate to a bomb signal.

The detection range of Scintex under the criteria already set forth is about 3×10^8 km, i.e., roughly solar orbital distances. This device is particularly sensitive to the interposition of a shield around the bomb which reduces not only the X-ray yield, but more unfavorably, reduces the temperature and therefore the spectrum of the radiation below the sensitive region of the fluor with its Be windows.

To overcome such shielding countermeasure effects, Panofsky originally proposed the use of an open photomultiplier system which would be sensitive to X-radiation of much lower energies. Under these conditions even shielded nuclear detonations could be detected at extreme ranges. Detectors of this kind under the name of Panex are being designed and tested, but because of their great sensitivity to many kinds of radiation, including low energy particles, they require much more study of their properties than the other types discussed.

The recent development of solid state radiation detection devices also shows great promise, but requires much research and development work to be done.

THE FULL SCALE EXPERIMENTAL SATELLITE

In line with a deliberate approach to a final operational surveillance system, carefully thought-out prototype satellites have been considered. Thus, a possible first satellite payload contains devices which are considered to be a first step in that direction, namely, portions of the above discussed conceptual detection schemes would be flown under test conditions in which further detailed information would be provided by so-called "support" experiments. In particular, these support experimental devices would be time correlated with the prototype detection units and with other phenomena, e.g., solar events in order to determine the preprototype response characteristics to the details of background events. This kind of information would then allow both operating characteristics to be changed (e.g., by command) or design changes to be made in later models. In addition,

information would become available on such parameters as the fraction of time that a particular range capability existed averaged over the lifetime of the system.

Later satellites would progress in the direction of less support experimentation and the extension of Gamex and Scintex toward the full systems. One would hope, therefore, to progress toward greater simplicity and more reliability in later satellites than in the first.

It is worth while mentioning here that the first satellite would have a logical decision system which would either transmit the ambient background data through a commutated, but otherwise continuous, data transmission system or, when an appropriate alarm signal appeared, switch to an alarm channel which would store the alarm data in a memory for later transmission as many times as interrogated.

A logical decision system is made up of many components which collectively allows prediction of a mean lifetime for the detection system. It would probably be necessary that the design be so adapted that there be a 90-percent probability of obtaining a 3-month lifetime for data reception.

The data from the detectors would be processed in the logics and then transmitted to the ground in digital form. On the ground the data would be recorded on tape and sent to a central data handling laboratory for reduction. In some cases, particularly early in lifetime of the satellite, the data would be used to provide new commands for detectors or other elements of the satellite.

TIME SCALE AND COSTS

The schedule below shows approximately how the payload costs for a program leading to three successful satellites might be distributed exclusive of any launch rocket, ground communication, or systems engineering changes. It is presently believed that under the most pessimistic assumptions 11 payloads would have to be fabricated for 3 successful launchings.

[Dollar amounts in millions]

Fiscal year	Number of payloads	Probe and piggyback experiment	Detector development and construction	Payload logics power and telemetry	Data reduction	Total
1961	1	$0.4	$1.0	$2.2	----------	$3.6
1962	6	.4	1.0	7.4	----------	8.8
1963	4	.4	1.0	5.4	$0.6	7.4
1964	----------	----------	1.0	1.4	1.2	3.6
1965	----------	----------	.5	.5	1.0	2.0

CONCLUSIONS

It is believed that the approach to the detection problem proposed is a conservative one with a high probability of success. At the moment effective countermeasures most likely to be successful are shielding of the nuclear detonation, but the rapid development of Panex type detectors together with a recommended theoretical program studying the effects of properties of shields should result in the maximum detection range staying at a few times 10^8 kilometers. Since the program is designed to take advantage of technical advances one may expect actual increases in range. It is to be hoped that a parallel program of studies on all aspects of the detection problem may turn up still other detection methods of greater range and simplicity. Thus it has always been hoped that electromagnetic radiation could be used, but no satisfactory theory of the generation of such signals in free space as yet exists.

It should be pointed out here that there is no intent to provide an operational surveillance capability in the approach discussed. Nevertheless, even the first successful satellite should have a "limited" capability although it might be via the detection of neutrons.

FACTORS AFFECTING TIME SCALE FOR ACHIEVING NUCLEAR DETONATION DETECTION CAPABILITY

(By Dr. Richard F. Taschek)

There has been discussed elsewhere a "deliberate" approach to the problem of providing information which would lead to an operational detection system. The time scale for this well thought-out plan requires about 2 years from start before a first full scale "experimental" satellite would be ready for launch and roughly 2 years more for full definition of a prototype operational unit. This time scale is largely determined by the complexity of the electronic decision-making elements if a system containing threefold detection sensors is to be used at maximum range and with an acceptable reliability criterion. It is further believed that during this period important information concerning interfering natural backgrounds in space can be obtained which will minimize the steps required to devise an operational detection system. Installation of the operational system might require at least 2 more years.

If on the other hand a reasonably assured early, but possibly limited, capability is desired, this can be obtained from ground based atmospheric fluorescence detection or from satellite based Argus-effect detection and possibly other less well-understood phenomena.

One should add to these the detection of neutrons from a satellite based device which might indeed already provide a limited surveillance in the first experimental satellite discussed above. It is believed likely that with the use of existing space communications networks, known logical circuitry for a neutron detector, a simplicity of the detector itself and an expected small background, a period of as short as 6 months might suffice for limited detection capability. It should be noted, however, that this method in common with the other two mentioned has a range of only about moon distances for a nominal 10 kiloton nuclear detonation.

The ground based fluorescence detection system could approach an operational capability in mid-calendar-year 1961.

These approaches directly to prototype systems imply continuing studies and improvements especially in the area of system reliability.

Representative HOLIFIELD. Our next witness is Mr. Carleton M. Beyer, of the Advanced Research Projects Agency, who will discuss the dollars and manpower for the underground and space systems.

Advanced Research Projects Agency has had the responsibility for improving the ability to detect and identify underground and space nuclear tests.

We appreciate your appearance here and realize that you have been held over some time.

STATEMENT OF CARLETON M. BEYER,[4] ADVANCED RESEARCH PROJECTS AGENCY

Mr. BEYER. It has just been a short trip between here and the Pentagon.

There are two parts of my presentation. In the first part I am to talk of the requirements in terms of dollars and manpower for the Geneva-type international control system, and the later part deals

[4] Born: San Diego, Calif., Aug. 25, 1915.
Education: Public schools, San Diego, Calif.; San Diego State College, San Diego, Calif.; Graduate School, Claremont Colleges, Claremont, Calif.
Married, two sons (10 and 8 years old).
Experience:
(1) July 1959 to present: Was Chief, Missile Engines and Propellants Branch and Saturn Project Manager in the Advanced Research Projects Agency, Office of the Secretary of Defense; now Assistant Director for Nuclear Research and Reliability.
(2) November 1957 to July 1959: Principal technical assistant to the Director, Office of Guided Missiles, Office of the Director of Defense Research and Engineering, Office of Secretary of Defense.
(3) November 1956 to November 1957: Chief of Engineering Division, Office of Guided Missiles, Office of Assistant Secretary of Defense for Research and Engineering.
(4) May 1946 to November 1956: Branch Head, Scientific Department, U.S. Navy Electronics Laboratory, San Diego, Calif.
(5) 1942 to May 1946: Research Associate for the University of California Division of War Research at San Diego and Key West.
Professional Societies: Senior member, Institute of Radio Engineers; member, Sigma Pi Sigma (physics); member, Phi Delta Kappa (education); member and consultant, American Ordnance Association.

with requirements for a coordinated program. With the permission of your staff I am adding to that a rather complete discussion of the work that is going on under Project Vela. So this first part is just limited to the Geneva control system estimates.

I have been asked to speak on the requirements in terms of dollars and manpower for the underground and space systems of a Geneva-type international control system.

The ARPA responsibility that was assigned by the Secretary of Defense and the Director of Defense Research and Engineering is for research and development to improve our ability to detect nuclear detonations underground and in space. ARPA is not responsible for the construction, staffing, or operation of a control system, but only for the underground and space research and development aspects.

Mr. RAMEY. When was ARPA given responsibility for the R. &. D. to improve the system?

Mr. BEYER. You mean to establish in some case a system?

Mr. RAMEY. Yes.

Mr. BEYER. September 2, 1959.

Mr. RAMEY. Who had that responsibility before that time, or was there such a responsibility?

Mr. BEYER. I believe in the memorandum of agreement between Dr. Killian, Dr. Quarles, and Mr. McCone of April 23, 1959, it was agreed that it would be split up in two different categories. Between that time and the time it was assigned to ARPA it was receiving various types of technical reviews within the Department of Defense.

Mr. RAMEY. You say ARPA is not responsible for the construction, staffing, or operation of a Control System. That presumably would be done by an international group.

Mr. BEYER. If it is an international group it would be agreed on at Geneva as to how this would be undertaken.

Representative HOLIFIELD. Your present responsibility does not include construction or staffing or operation of the Control System?

Mr. BEYER. Yes. We are a research and development outfit.

Representative HOLIFIELD. You are strictly confined to the research and development of hardware?

Mr. BEYER. And techniques.

Representative HOLIFIELD. Detection type hardware.

Mr. BEYER. Yes.

Representative HOLIFIELD. And detection techniques.

Mr. BEYER. Yes, sir. And system techniques.

Representative HOLIFIELD. In your computation that you gave here in terms of dollars, is it confined to that scope or do you take in in your testimony an estimate of the whole field?

Mr. BEYER. We are reporting in this testimony on a study done by someone else to be responsive to a request for this information, and it does cover the overall field.

Representative HOLIFIELD. I see.

You may proceed.

Mr. BEYER. For assistance in this hearing, estimates have been obtained from previously prepared studies that reflect the general magnitude of the cost and effort required to establish a control system for detecting nuclear detonations underground, in the atmosphere and at high altitude. These estimates are likely to be affected by further negotiations at Geneva and by the cost of R. & D. on space

satellites or the use of space satellites; nor did they include R. & D. on underground detection. The estimates presented may be reduced if detection technique breakthroughs are achieved as a result of the research and development under Project VELA in the Department of Defense, or they may be increased if techniques for concealing nuclear detonations are improved.

Representative HOLIFIELD. Why do you say that you have the responsibility for research and development to improve our ability to detect nuclear detonations underground, and then you say that the estimates would be affected by the cost of R. & D. on space satellites and the use of space satellites?

You also say that the estimates did not include R. & D. on underground detection.

Mr. BEYER. Sir, if I may explain that using different words, I probably did not choose good ones here, the R. & D. responsibility of ARPA is for R. & D. on the underground environment, skips over the atmospheric area, and into space. However, the study I am going to try to summarize here is concerned with underground detection and the detection of relatively low altitude or atmospheric type of detections. But at this time we do not have any completely reliable estimates for the space detection requirements.

Representative HOLIFIELD. I see. I think that clarifies it.

Mr. BEYER. It should also be understood that since a number of details regarding the control system have not been agreed on at Geneva, the estimates cannot be precise and are likely to be changed significantly as a result of further negotiation.

Representative HOLIFIELD. Would you say that paragraph was entirely accurate? Wouldn't you say that not only of a number of details have not been agreed upon but a number of the primary objectives have also not been agreed upon?

Mr. BEYER. I would agree. For example, the United Electrodynamics studies, which related to the use of large arrays, were discussed. If we were to go to such a system and it was agreed upon, it would represent a major change in the type of system to be used.

Representative HOLIFIELD. Of course, the suggestion of 600 stations in place of 21 stations is hardly a detail.

Mr. BEYER. I agree.

Representative HOLIFIELD. The number of inspections, of course, is not exactly a detail.

Mr. BEYER. I agree.

Representative HOLIFIELD. That seems to be a pretty important matter to both sides, I would say.

Mr. BEYER. I quite agree.

In order to install and operate a worldwide control system quickly and effectively, the proper detection equipment, an efficient organization with excellent interunit communications, and clear, previously agreed upon rules and procedures are required.

If implemented the overall responsibility for the control system would be exercised through a multination control commission to which the Administrator for the Control System would report. He would be located at system headquarters.

Headquarters: The system would be controlled from Headquarters where a data analysis center, a research office, and a central laboratory would be located. Also several staff offices would be required.

Regional offices: Regional or intermediate offices are envisioned for the administrative supervision and logistic support of control posts. They would not, however, be responsible for the direct control of actions in the control post.

Control posts: In areas subject to few earthquakes, control posts would be spaced at 1,700-kilometer intervals.

Where there is considerable seismic activity, the control post spacing should be about 1,000 kilometers. In ocean areas, the ship and island control posts would be spaced between 2,000 and 3,500 kilometers apart.

Number of control posts:

On continents	110
On large oceanic islands	20
On small oceanic islands	40
On ocean vessels	10
Total	180

The techniques employed in a control post would consist primarily of sensing devices or instruments that record disturbances of the earth or atmosphere. The recordings of several techniques and several control posts are analyzed and compared, to determine whether a disturbance was natural or manmade. Such a determination is not always possible, particularly if the disturbance is of low magnitude or low yield. To assist in this determination, inspection teams will be used to investigate sites of suspected nuclear explosions. The inspection teams should be located at selected regional offices and land control posts in such a way that the inspection teams would have prompt access to suspected sites and should be equipped to complete the inspection. The number and makeup of the teams is still under discussion at Geneva.

Aircraft are to be used that are equipped with devices for taking samples of atmospheric debris. Similar devices are also located at each control post. These samples are then analyzed to determine if they include any nuclear debris and, if so, if such nuclear debris is of recent origin.

The system described so far does not provide for detection of nuclear detonations or R. & D. for space detection, particularly the use of statellites, as is being studied by ARPA. Also, further research is in progress on underground detection. I will cover these points in more detail later.

Communications system: Rapid and reliable communications will be required to transmit data messages to the Headquarters. These requirements will probably necessitate the use of radio.

Personnel: It appears that a limited number of technically trained personnel is available for many of the positions in the System. An extensive training program would be required. Since a number of nations would be involved in the Control System, all rules, procedures, and communications would have to be designed so that the several languages would pose no barrier.

It is estimated that approximately 20,000 people may be required to establish and operate the System, excluding space detection. The great majority of these people will be required in the control posts as technicians and as support personnel. A control post would have approximately 30 to 35 technicians with number of additional support

personnel that would depend on the remoteness of the post from support resources. The System headquarters may require 1,500 to 2,000 personnel, the majority to be employed in the analysis of data submitted by the control posts. Other personnel requirements will be in the fields of air operations, regional offices, ships, and communications.

Representative HOLIFIELD. Is there any thought at all that we have the type of trained personnel in this field to fill a complement of 20,000 employees?

Mr. BEYER. They are not available at this time in that number.

Representative HOLIFIELD. This would involve a very extensive training program in the field of seismology and electromagnetic readings of devices, maintenance of devices.

Mr. BEYER. That is right.

Representative HOLIFIELD. Calibration and all that sort of thing.

Mr. BEYER. Yes, sir. However, I believe there are people with technical background in this country who could be retrained to fill these positions, and I believe among some of the other countries there are nuclei that could be expanded with a training program.

Representative HOLIFIELD. Would you anticipate any trouble in obtaining people to work in some of these far-off locations and be stationed, let us say, in the Antarctic and Siberia and the remote areas of the land mass of Red China, Mongolia, Australia, South Africa? Do you think there would be any difficulty in getting people to go out there and live under the primitive conditions they would have to live under?

Mr. BEYER. I think undoubtedly there would be some difficulty. I am personally not qualified to discuss how the other nations in particular would resolve their manning questions in this connection.

Representative HOLIFIELD. These teams, though, would be international teams?

Mr. BEYER. That is correct.

Representative HOLIFIELD. Therefore, wherever a team was stationed, it would be assumed that there would be three types of personnel from the different nations.

Mr. BEYER. That is right.

Representative HOLIFIELD. I think the complement would be fairly well divided between Great Britain, United States, and the Soviet Union and a certain number from other nations, is that right?

Mr. BEYER. Yes.

Representative HOLIFIELD. So it has been discussed in the negotiations.

Mr. BEYER. Yes. My feeling on this is that if we are able to get people to man our own DEW line stations at very remote points in the world, we can with proper rotation planned, proper facilities on the base and some financial inducement, find good people to do this job.

Representative HOLIFIELD. Are most of these people on the DEW line Defense Department career people?

Mr. BEYER. No, sir. Most of them are contract people.

Representative HOLIFIELD. Most of them are contract people, civilians?

Mr. BEYER. Yes, sir.

REPRESENTATIVE HOLIFIELD. How many DEW line posts do we have, approximately?

Mr. BEYER. I am not qualified to discuss the details of that. That is out of my field.

Representative HOSMER. That is a matter that would be controlled internationally, such as living conditions and everything else. You would have no idea until such time as specific minimum requirements for housing and food and so forth were established.

Mr. BEYER. Yes. I would expect them to require premium pay of some sort to induce good topnotch people to man these stations.

Representative HOSMER. You mentioned personnel requirements for operating these things. Did you make any inclusion there for personnel to maintain the integrity of the communication system?

Mr. BEYER. This would be part, as I understand it, of the technical complement of from 30 to 35 technicians, but would not be taken from the additional support personnel. In other words, they are counted in this.

Representative HOSMER. We had talk yesterday of some 20,000 miles of telemetering cable being involved. I think it was in the U.S.S.R. alone. Those people certainly would not give very much guarantee to the integrity of those systems, would they?

Mr. BEYER. In this instance, we are talking about a radio link, so the integrity would have to be controlled at the post and at the receiving end where the data are converted back into useful information.

Representative HOSMER. That is in your mind the responsibility of the people who man a particular post?

Mr. BEYER. Yes. This, again, is one area where I do not believe there is complete agreement as to exactly how communications will be carried on and through what type of system and with what priority. Hence, as I will point out later, the estimates which I will give are rather broadbrush or corresponding estimates that will have to be refined as these, as I have called them indirectly actually, details are further spelled out.

Representative HOSMER. It is important that the communications be as reliable as the detection machinery?

Mr. BEYER. Yes, sir. Not only reliable but very rapid.

Now with regard to cost, it is estimated that the International Control System without satellites, will cost the participating governments in the neighborhood of $1 billion to establish and $250 million a year to operate.

I would like to point out that these are well-rounded figures and are based on the deployment I have previously discussed.

It would cost the United States from one to several million dollars to establish a control post depending upon the climate and support resources available locally. The final siting of a control post will, of course, determine the cost. The final site cannot be determined until detailed map studies and visual and instrument surveys have been made and the results analyzed. In the establishment of a control post,we must consider such things as:

Building materials, construction, fuel storage, heating facilities, pole lines, electric power, water wells, communications, technical equipment, housing, dining facilities, transportation, initial supply and resupply, and roads.

In some cases, control posts will have to be completely self-sufficient. The annual operating costs will consist largely of salaries but will cover, also, such items as resupply of technical and other equipment and supplies, transportation, research and development, and training.

Space-based detection system: I can only speak in general terms about the space-based operational detection system, since the detection capability requirements have not yet been established for this environment. As Dr. Taschek just mentioned, we have just completed on a joint basis between NASA, BMD, and AEC, a study for an R. & D. program which is aimed at getting answers to some of these questions before a system can be defined precisely and costed.

The detection effectiveness of satellite-borne detectors is a function of the instrumentation's sensitivity and ability to discriminate between signals produced by a nuclear detonation and by natural background phenomena. It is possible to obtain various limited levels of detection capability over the next few years. However, by conducting necessary research and development, detection effectiveness can be rapidly improved with time.

Surface-based high-altitude detection system: A capability to detect nuclear detonations to a limited distance from the earth can be achieved by using detection instruments located on the ground. These instruments would be located at the control posts if such a system is implemented.

Underground: The resources estimates I have indicated and the system I have described includes underground detection techniques based on the present state of the art.

In summary: Approximately 20,000 people would be required to operate a control system capable of detecting underground, surface, and atmospheric detonations. The cost of establishing the system, would be approximately $1 billion, take about 6 years to establish, and the annual operating cost would be roughly $250 million. This is a hopeful statement. All these costs would be shared by the participating countries. These estimates specifically exclude outer space detection that requires the use of satellites.

Representative HOLIFIELD. This is based on 180 control posts worldwide?

Mr. BEYER. Yes, sir.

Representative HOLIFIELD. That would be something like 37 in Asia?

Mr. BEYER. On that order; yes, sir.

Representative HOLIFIELD. About 21 of which would be in the Soviet Union.

Mr. BEYER. In the U.S.S.R.; yes, sir.

Representative HOLIFIELD. So if we magnified this on the basis of 21 stations up to 600 stations, as suggested by Dr. Bethé, this would go up by a factor of 30.

Mr. BEYER. I don't know if we can extrapolate directly in this case because there would be some common costs. But certainly it would go up many times. It is a very complex type of estimate to make, sir.

Representative HOLIFIELD. I assume that this is also based on what they call the low frequency seismographs.

Mr. BEYER. Yes.

Representative HOLIFIELD. And not only high frequency as suggested by Dr. Roberts and Dr. Bethé.

Mr. BEYER. I think all I can say on this point, and Dr. Romney should talk in detail about this later, that this is based on a 21-seismometer installation with 10 in an array.

Representative HOLIFIELD. Ten in an array?

Mr. BEYER. Yes, sir. This is much less than the thing you heard about yesterday.

Representative HOLIFIELD. Yes. We were asked for at least 70 to 100 in an array.

Mr. BEYER. Yes, sir.

Representative HOLIFIELD. This is based on 10 in an array.

Mr. BEYER. And two matched horizontal component instruments and three, three-component instruments. So it is much less in terms of the number of seismometers that would be involved than the study you heard reported yesterday.

Representative HOLIFIELD. Mr. Hosmer?

Representative HOSMER. No questions.

Representative HOLIFIELD. Mr. Ramey.

Mr. RAMEY. How much money has ARPA expended since it assumed its responsibilities for research and development on detection in September of 1959?

Mr. BEYER. Would you want the answer now? I plan to have it as part of my presentation in a later portion.

Representative HOLIFIELD. Are you going into that aspect of it?

Mr. BEYER. Yes, sir. We will cover the dollars as well.

Representative HOLIFIELD. Will you also be going into the number of contracts that have been let and projects undertaken, moneys committed, moneys available? Will you cover that field later?

Mr. BEYER. I can cover that. I will purposely stay off the names of the contractors because there are some that are being negotiated at the present time.

Representative HOLIFIELD. That is understandable.

Are there any further questions?

If not, thank you very much, Mr. Beyer, for your presentation. It has been very helpful.

Mr. BEYER. Thank you.

Representative HOLIFIELD. How long will it take you on your second statement, Mr. Beyer?

Mr. BEYER. I would say on the order of 40 minutes, with questions.

Representative HOLIFIELD. Then we will reconvene at 2 o'clock.

(Whereupon, at 12:10 p.m., the committee was recessed, to reconvene at 2 p.m. the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order. This afternoon's session will open with further testimony from Mr. Carlton M. Beyer of the Advanced Research Project Agency who will testify on general research program requirements for any agreed-on period of cooperative research. I hope you will not limit yourself, sir, to the word "cooperative." I hope you will also comment on a coordinated program of research and an independent program of research for the United States.

You may proceed.

STATEMENT OF CARLTON M. BEYER,[5] ADVANCED RESEARCH PROJECTS AGENCY—Resumed

Mr. BEYER. Thank you.

Mr. Chairman, my statement is in two parts. The first part is some general remarks about program requirements for a coordinated research program. The second part is a brief summary of the overall work going on under Project Vela.

I have been asked to discuss the requirements for a program of cooperative research. Such a program was mentioned as a coordinated research program in the statement recently issued by President Eisenhower and Prime Minister Macmillan.

The words "coordinated research" are being used to describe a concept that has been proposed under which each participating nation would let the others know of its plans and experiments, share data from these experiments, and compare results obtained from them. Under a coordinated program as currently envisaged, one nation would not approve another nation's plans, experiments, or resulting reports. Each participating nation would keep the other informed of all tests and make it possible for others to observe and participate in such tests. The coordination between participating nations could be accomplished by an information exchange group made up of technical and scientific personnel who might report to the Control Commission. This approach would have to be considered and approved at Geneva.

Joint research tasks are not excluded from the concept of a coordinated research program. However, it should be noted that joint efforts would require the approval of the participants with the result that the administrative procedures would probably be more time consuming than those associated with a coordinated program.

Representative HOLIFIELD. At that point I want to compliment you upon this approach. I feel it is very important that the U.S. program of research and development be vigorous and aggressive and not subject itself in any way to any type of obstructive or stalemating procedures or practices or obstructions.

Mr. BEYER. Thank you.

Underground detection improvement is receiving active consideration under the coordinated approach. There is no comparable program being considered for the detection of nuclear detonations on the surface or in the atmosphere because the detectability of those blasts at present is considered fairly satisfactory. However, some additional research and development effort will be required before an adequate capability of detection of nuclear detonations in space can be assured. The problem of detecting high-altitude nuclear detonations can be separated into two parts: the first involves the use of detection intruments located on the surface of the earth, and the second involves the use of space-based or satellite-borne detectors.

Representative HOSMER. When you speak of high-altitude detonations are you encompassing all detonations in space or just within a certain range of the earth?

Mr. BEYER. Let us say from 50 kilometers from earth to as far as we can reasonably go. I believe some of the scientists have talked about the limitations of the present schemes for detection which will require further research and development to verify and improve.

[5] See biographical sketch, p. 367.

Representative HOSMER. You have not added an intermediate zone here. You are just talking about two zones.

Mr. BEYER. That is right.

Now that the various elements of the coordinated program have been mentioned, I would like to comment briefly on how the current U.S. research and development program is being undertaken. Two developmental approaches are being pursued concurrently. One approach consists of methodical investigation across broad technical fields with the objective of yielding results in the nature of major technological breakthroughs over a period of years. The other approach consists of immediate acceleration and emphasis of work on selected problem areas and is designed to yield many significant improvements of a specialized nature within the next year or two. Under the latter approach, duplicate effort would be placed on areas in which early improvements are most likely.

In undertaking a research and development program involving underground and high-altitude detection, it is essential to consider the type and availability of the technical talent that can assist in making rapid progress. Use is being made of many highly qualified scientists and engineers in university, industry, and Government organizations. Increased use will be made of many other experts and specialists in related technical fields.

I would now like to summarize Project Vela, which is the current U.S. program of research and development being undertaken to improve the detection of nuclear detonations occurring under the ground and in space.

The basic responsibility for this program resides in the Department of Defense, and is being conducted in cooperation with the Atomic Energy Commission, the Departments of Commerce and Interior, and the National Aeronautics and Space Administration.

Within the Department of Defense, Project Vela was assigned to the Advanced Research Projects Agency on September 2, 1959. The project is divided into three parts, and the following names have been assigned:

Vela Uniform for the detection of underground detonations.

Vela Sierra for surface-based detection of high-altitude detonations.

Vela Hotel for space-based detection of high-altitude detonations.

Vela Uniform: The first task is being conducted by the Coast and Geodetic Survey and is intended to provide a source of standardized quantitative seismic data on a worldwide basis. This will be accomplished by providing a number of the world's seismological research stations with standard, calibrated instruments, to be maintained and operated by them as part of their normal routine. The data accumulated at these stations would be exchanged freely among seismologists throughout the world. To accomplish the first step in this endeavor, the National Academy of Science has established a committee of eminent seismologists, who are currently preparing a report recommending the specifications for the standard equipment. The U.S. Coast and Geodetic Survey of the Department of Commerce will procure this equipment and arrange for its distribution. Procurement and distribution is expected to commence in midyear for the first 50 sets of equipment.

Task 2 entails the establishment of a large-scale basic research program on the generation and propagation of seismic waves. This is deliberately intended as a stimulation of basic research in the area of seismology, with eventual long-term benefits to the detection problem. Task 2 also consists of a project of a somewhat more applied nature. This aspect involves the investigation of new types of seismic instruments as well as their use in complex arrangements and in locations such as deep in the earth's crust.

Under task 3, in order to make an immediate attack on the requirements for and the detailed characteristics of a system of stations intended for the detection of underground nuclear detonations, it is planned to build and operate two prototype detection stations: one based on the equipment recommended by the Geneva Conference of Experts, the other containing the improvements recommended by the Berkner Panel. Although these stations are expected to serve as prototypes for a final system, they will also be used as the stations for the trial application of new techniques resulting from more basic elements of the program, and for the study and application of refined system techniques such as data processing and analysis.

In task 4, a series of underground nuclear and high-explosive detonations have been recommended to provide research data on underground explosions and to test components of a possible control system. These should be conducted in a variety of geological environments, calculated to increase our understanding of the variation in seismic signals resulting from underground weapon tests.

In addition to the long-term program, specific short-term studies are nearing completion on the following subjects:

(*a*) A review of the feasibility of utilizing unmanned seismic stations to extend the capabilities of a detection network.

(*b*) A systematic review of earthquake statistics, and the criteria for differentiating between earthquakes and explosions.

(*c*) A study of the feasibility of utilizing large arrays of seismographs to increase the detection sensitivity of a detection station. This last was summarized for you yesterday afternoon.

At present the fiscal year 1960 program for Vela Uniform involves $8,695,000 which includes $2,310,000 for planning test shots, if approved.

The entire program is in consonance with the objectives given in the Berkner report. It has been reviewed by the Ad Hoc Group on Seismology formed by Dr. York and more recently by the Ad Hoc Panel on Seismic Research and Development chaired by Dr. Panofsky.

This most recent review resulted in suggestions for improving the accelerated Vela Uniform program. Plans for implementing these suggestions in fiscal year 1960–61 are under active consideration.

Vela Sierra: The second part of the Vela project, known as Vela Sierra, consists of research and development aimed at improving the detection of high altitude nuclear detonations using instruments located on the earth's surface.

Several techniques for the detection of high altitude detonations with instruments located on the surface of the earth are potentially available; however, there is a need for additional research and development to develop these into useful components of a detection system. Two of the promising areas involve the use of optical and electro-

magnetic methods. I will mention these briefly since the technical experts have or will discuss them in greater detail during these hearings.

Optical methods: There are two techniques available which depend on the detection of visible light. One measures the light emanating directly from the explosion. The other measures the fluorescence or glow of the upper atmosphere which results when the radiation from the nuclear detonation interacts with atmospheric molecules.

Representative HOSMER. One of the gentlemen who testified yesterday indicated that the length of the emission of light from an explosion in space would be for a matter of microseconds.

Mr. BEYER. Very short duration.

Representative HOSMER. Of course, it is too short for eyeball visibility, I presume.

Mr. BEYER. Yes.

Representative HOSMER. It would have to be caught electronically.

Mr. BEYER. Yes, by other than human means. It would have to be through some sort of equipment capable of reacting to the short pulls.

Although the basic phenomena which occur in each case are relatively well known, the effects of the natural phenomena must be better understood before detection instruments can be developed which are reliable enough to be used in a control system. Specifically, lightning flashes, the aurora, and the presence of the clouds which obscure the view of the instruments, will adversely affect their performance.

Electromagnetic methods: The detonation of a nuclear weapon is accompanied by the emission of a radio signal. Detection of this signal by a suitable radio receiver appears to be an attractive possibility. Before this signal can be used, two things must be better understood: First, there has to be a better understanding of the frequency and time characteristics of the emitted signals; secondly, the natural atmospheric noise generated by lightning storms has close similarity to the signal that is to be detected and methods for discriminating between the detonation signal and the natural background must be devised.

The propagation of radio signals between points on the surface of the earth or from outer space is significantly affected by the ionized regions of the upper atmosphere, called the ionosphere. After a nuclear detonation has taken place, radio signals may be absorbed, the direction of propagation may be changed and the signal may be scattered depending on the degree of ionization, its distribution in space and the frequency of the radio waves. In principle, these modifications to radio propagation conditions can be detected and used as an indication that a nuclear detonation had occurred; however, natural perturbations also occur in the atmosphere. These natural perturbations result from solar flares, meteors, and auroral activity. Before this approach can be used, it will be necessary to understand more about natural variations in the ionosphere and radio signal propagation conditions.

With the examples that I have discussed I have tried to show several of the approaches being considered under the Vela Sierra program which is just getting underway. Funds for commencing this work were received 2 weeks ago and are in the amount of $1,050,000. Additional work is planned.

Representative HOLIFIELD. Is this last section, the electromagnetic method, the Vela Sierra program?

Mr. BEYER. These are examples. This does not describe all of the parts of the program. These are examples which we are sure we are going to undertake.

Representative HOLIFIELD. You may proceed.

Mr. BEYER. Thank you.

Vela Hotel. The third part of the Vela project is known as Vela Hotel. This portion of the project consists of research and development associated with space-based techniques for detecting nuclear detonations which occur in outer space beyond the effective range of the surface-based detectors just discussed under Vela Sierra. The method most likely to be employed requires the detection of signals generated by detonations in space with special instruments mounted on satellites, and then relaying the information to ground stations.

At present, the use of X-ray, gamma-ray, and neutron detectors appears to offer the best approach for early development of a practical operational system. The ability to detect a nuclear detonation by this method is dependent on at least the following factors:

(*a*) The amount of radiation emitted from a nuclear detonation.

(*b*) The distance between the detonation and the detection system.

(*c*) The natural radiation background or environment in which the detection system is operating.

(*d*) The sensitivity of the detection system, sensors, and associated electronic instrumentation.

(*e*) Any shielding or other countermeasures which might be employed to conceal the nuclear detonation.

In order to be able to build and operate an effective space-based detection system, it will be necessary to investigate each of these factors. A research and development program oriented to accomplish this would include:

(*a*) Study and examination of the basic mechanisms of nuclear weapon detonations in space. This will result in a refinement of our understanding of the detectable radiations emitted and may lead to the discovery of new and more effective detection methods.

(*b*) Determination of the natural radiation background in space where satellite-borne detection systems are to be located.

(*c*) A large effort on the improvement of X-ray, gamma ray, and neutron sensing devices with particular emphasis on advanced techniques.

(*d*) In conjunction with the work on detectors, considerable effort on the electronic equipment which analyzes the signals detected. This signal analyzer, or logics as it is sometimes called, provides the capability of discriminating against natural background signals.

(*e*) Research on promising detection schemes other than those derived from sensing X-ray, gamma ray, and neutron signals. Included in the approaches to be investigated are the detection of air fluorescence, direct visible light, other electromagnetic signals such as radio waves, and light scattered by the bomb debris. The objective of this work would be to devise improved techniques which would lead to more effective detection capability.

(*f*) Thorough investigation of all aspects of equipment reliability. The objective is to increase the useful operating time of the equipment. This will tend to reduce program costs since the required

testing and acquisition of data can be achieved with fewer replacement parts and in less overall time.

(*g*) Study and research for countering any techniques for concealment which might be utilized to mask a nuclear detonation from a surveillance system.

I have made no reference to the space vehicles required to place the detection payload into orbit. Vehicles developed under existing programs appear to be suitable. The information regarding Vela Hotel which I have just discussed was derived from a report recently prepared by a technical working group established by ARPA. Participating on the working group were technical personnel from the USAF Ballistic Missile Division, Atomic Energy Commission, National Aeronautics and Space Administration, and associated contractors. The coordinated report was completed roughly 2 weeks ago and is now undergoing review by the agencies involved.

A decision with regard to action to be taken on this program will be made in the near future. In the meantime, the Department of Defense has initiated the necessary planning in conjunction with the AEC and NASA to flight test appropriate experimental gear as piggybacks on forthcoming DOD and NASA space probe and satellite flights.

SUMMARY

In summary, I would like to say that ARPA, in the Department of Defense, is vigorously prosecuting the Vela project in conjunction with the AEC, NASA, the Departments of the Interior and Commerce, and with substantial assistance from universities and industry. The Vela Uniform program is well underway. The Vela Sierra program is funded and just getting started. A comprehensive study has been made of the Vela Hotel program and the resulting report is being actively reviewed by top-level Department of Defense, Atomic Energy Commission, and National Aeronautics and Space Administration personnel.

Representative HOLIFIELD. Thank you, Mr. Beyer, for your statement.

Mr. Beyer, I want to go back into a little history. Over a year ago the Berkner report on possible improvements and necessary research and development to accomplish it was issued. Dr. Bethé testified before us at that time along with Dr. Berkner and his associates and indicated that we could expect considerable improvement in detection and control. Yesterday Dr. Bethé indicated some disappointment in that not much progress had been made in the last year. I want to emphasize that I feel the same way as Dr. Bethé on that point. I don't blame you, of course, for this situation. I have been told that you are an ardent advocate of more research and development.

At this point, I would like to put into the record some inquiries the Joint Committee has made during the last year as to programs on research and development in this field. These are letters from the chairman, Senator Anderson, to Dr. James R. Killian of June 25, 1959, a copy of which was sent to the Secretary of State. The answer of Dr. Killian to Senator Anderson on July 2. An additional letter from Dr. Killian on July 14. These all have to do with the research program in this field.

(The letters referred to follow:)

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
June 25, 1959.

Dr. JAMES R. KILLIAN, Jr.,
Special Assistant to the President for Science and Technology,
The White House, Washington, D.C.

DEAR DR. KILLIAN: On June 12, 1959, with concurrence by the Department of Defense, the Atomic Energy Commission, and the Special Assistant to the President for Science and Technology, the Department of State released to the public a summary of the conclusions of the Panel on Seismic Improvement. The first and summary section of a report on the need for fundamental research in seismology by the Panel was also released at the same time.

"While the Panel has made no detailed estimates of support for seismology at present, it is believed to be less than $500,000 annually excluding private research in petroleum exploration. This is scarcely enough to carry on basic observations, and permits no very rapid or significant progress in the science. The Panel recommends that this be increased to about $6,575,000 for the first year based on studies of particular potentialities, with an increase to about $10,520,000 in the second year."

Dr. Berkner also stated that the Panel was of the opinion that American science can launch this effort successfully.

In view of the extreme importance of this matter, particularly at this time, when we have in effect a nuclear test moratorium and are negotiating with the U.S.S.R. and Great Britain for a test cessation agreement to include a reliable detection and inspection system, I believe it is of urgent necessity to undertake the required support of seismology. On the basis of Dr. Berkner's March 31 letter, and the recommendations of the Panel, I believe it is urgent that the United States immediately commit the necessary money to support research and development in seismology at the level recommended.

I would appreciate it if you would advise me as to what action is being taken to comply with these recommendations. In the event additional appropriations by the Congress is required, please inform me as soon as possible in order that no delay may occur. I would also appreciate knowing if the level of support recommended in Dr. Berkner's letter is classified and, if so, the reason therefor.

Because of their interest and responsibilities in this matter, I am sending a copy of this letter to the Secretary of State, the Secretary of Defense, and the Chairman of the Atomic Energy Commission.

Sincerely yours,

CLINTON P. ANDERSON, *Chairman.*

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
June 25, 1959.

Hon. CHRISTIAN A. HERTER,
Secretary of State,
Washington, D.C.

DEAR MR. SECRETARY: I am sending to you attached a copy of a self-explanatory letter which I have sent this day to Dr. James R. Killian, Jr., Special Assistant to the President for Science and Technology, concerning recommendations by the Panel on Seismic Improvement as to the need for greater support for seismology.

The attached letter is classified secret because it quotes from a letter of Dr. Lloyd V. Berkner, chairman of the Panel on Seismic Improvement, which letter was so classified.

Sincerely yours,

CLINTON P. ANDERSON, *Chairman.*

THE PRESIDENT'S SCIENCE ADVISORY COMMITTEE,
EXECUTIVE OFFICE BUILDING,
Washington, D.C., July 2, 1959.

Hon. CLINTON P. ANDERSON,
U.S. Senate,
Washington, D.C.

DEAR SENATOR ANDERSON: This is the first of a two-part response to your letter of June 25, about the recommendations of the Panel on Seismic Improvement. I am bringing together the facts about the extent to which a research program has been initiated in response to the recommendations of the Panel and will comment upon that subject in a second letter.

You ask whether the level of support recommended in Dr. Berkner's letter is classified and if so, the reason therefor. In answer to this, I enclose a public release of the State Department, dated June 12, entitled, "Summary Reports of the Panel on Seismic Improvement." Accompanying this release is a summary document entitled, "The Need for Fundamental Research in Seismology," and you will note on pages 13, 14, 15, and 16, a discussion of the requirements for an augmented seismological research program. The entire report on the need for fundamental research has been declassified. I understand the State Department is currently preparing copies of this whole research report for publication.

I will report to you further at the earliest possible time.

Yours sincerely,

J. R. KILLIAN, Jr.

THE PRESIDENT'S SCIENCE ADVISORY COMMITTEE,
EXECUTIVE OFFICE BUILDING,
Washington, D.C., July 14, 1959.

Hon. CLINTON P. ANDERSON,
U.S. Senate,
Washington, D.C.

DEAR SENATOR ANDERSON: As anticipated in my letter to you of July 2, I am completing my response to your request for information about the recommendation of the Panel on Seismic Improvement which deals with increased support of research in seismology.

In April of this year the Department of Defense assumed primary responsibility for initiating the expanded research program in seismology directed toward the fundamental problems involved in the detection and identification of underground nuclear tests as recommended by the Panel on Seismic Improvement. The Department of Defense has informed me that this responsibility has been assigned to the Air Force Technical Applications Center (AFTAC), a group that has had continuing programs in the past for both pure research in seismology and systems engineering in closely related fields.

This group is now completing its plans for implementations of the recommendations of the Panel on Seismic Improvement; the necessary funding and executive decisions should be forthcoming in the near future. AFTAC has worked in the past with other Government agencies, such as the Coast and Geodetic Survey and the Geological Survey, on fundamental seismological research and would expect to implement part of the expanded research program through those agencies. I would suggest that you contact the Department of Defense for further information on the present research programs of AFTAC and the plans that are being made to follow up the Panel recommendations.

As you know, the Atomic Energy Commission has announced plans to conduct a series of nonnuclear, high explosive tests in order to obtain information relating to the theory of seismic decoupling. This program will implement one of the specific recommendations of the Panel on Seismic Improvement. These high explosive firings are expected to commence late this summer. Instrumentation will be carried out by the Coast and Geodetic Survey and by AFTAC. Additional details on these plans can be obtained from the Atomic Energy Commission.

The other aspect to this question is the support for seismology being provided by the National Science Foundation. In response to the report of the Panel, the Foundation has asked its Advisory Committee on the Earth Sciences to review the present status of this field in the United States and to recommend measures to stimulate university and research interest. It is also calling together representative seismologists to explore new research opportunities. The Foundation is prepared to expand its support in this field which is currently (fiscal year 1959) on the order of $290,000. I should point out that the National Science Foundation

does not solicit research proposals, but restricts itself to choosing among alternative proposals which appear before it. If more proposals came from seismologists, more seismological research could and will be supported. The Foundation expects that the increased interest in seismology will, in fact, lead to an increase in the proposals in this field during the coming year, though we may find that the United States has a real shortage of people expert in this branch of science. One of our problems will be to educate more seismologists at the same time as the necessary research is carried out.

In addition to this support, the National Science Foundation will support the publication of the full report of the Panel, and thus aid in the dissemination of this important information and stimulate research proposals in this field.

Sincerely,

J. R. KILLIAN, Jr.

Representative HOLIFIELD. I think you have outlined in your statement here a pretty comprehensive program of research and development. I wanted to explore the status of this program and also the amount of money that was available and the amount of money expended and so forth.

First, you do have some money available, do you not?

Mr. BEYER. Yes, sir. I wonder if before we get off on the money question I could make a few general comments about what has transpired since the Berkner report was made available in early March 1959.

Representative HOLIFIELD. I believe that was brought out by my questions.

Mr. BEYER. I believe your letters certainly are important milestones. I have not read them. But I am sure that they will indicate the interest and the responses received. However, there may be some value on the part of the record to show some of the things that have gone on before ARPA was put into the responsibility for the Vela problem. If you care to, I could summarize one or two highlights.

Representative HOLIFIELD. Go right ahead.

Mr. BEYER. As I understand it, the report from the Berkner Panel was dated on the 16th of March.

Representative HOLIFIELD. Of 1959.

Mr. BEYER. Yes, sir; 1959 is the version I have. Then after receiving this report it was given policy level consideration, and as I believe I mentioned before the recess, on the 23d of April, Dr. Killian, Mr. Quarles, and Mr. McCone met and they had a general discussion with regard to the organization and assignment of responsibility. This, then, became one of the policy documents which assigned responsibility in part to the Department of Defense and another part to the Atomic Energy Commission. Then a series of technical reviews took place to discuss ways and means of implementing the recommendations made by this panel and the Panofsky Panel.

In connection with this, the Air Force Technical Applications Center came up with an initial plan which was dated in early July 1959. Then subsequent to this, this was reviewed by a panel formed by Dr. York called the Ad Hoc Group on Seismology, which was headed by Dr. Press of California Institute of Technology.

Representative HOLIFIELD. What was the date of that?

Mr. BEYER. They had two meetings, sir, and I don't know the exact dates of those meetings offhand. But they took place prior to the assignment of the problem to ARPA. So it would be between June and September of 1959. There were two meetings of the seismic

group and one meeting of the high altitude group which was headed by Dr. Panofsky.

I might add that the groups are being reconstituted at the present time under a single chairmanship and Dr. Richard Latter will be the chairman of the reconstituted group, which will be responsible for looking at the overall nuclear detection problem.

This ad hoc group then will bring to bear topnotch scientific talent to advise the Department of Defense on matters relating to Project Vela.

Then on September 2, 1959, the Secretary of Defense assigned responsibility for project Vela to ARPA. On October 2 we received some money and under ARPA Order 104 we issued $825,000 at that time to AFTAC. In addition, two short-term studies were set up at about the same date. Then after this initial work got underway, on December 11, the Bureau of the Budget, in response to a Department of Defense request, transferred $7.5 million from the defense emergency fund to ARPA account. The AFTAC people came up to review the seismic portion of the program which was the part for which these funds were made available. They reviewed the program before us on January 29, 1960. It was approved with minor modifications, on February 4, 1960. So the money has been made available to AFTAC for prosecuting the program.

Now with regard to your question on the money, I have some information in summary form on the status of the various parts of the problem, the money available and the money actually obligated. Obligated in this instance, in some cases, will mean that the contract is signed and people are working. In other cases, it is under negotiation. I cannot split those two categories. But the money is out to the procuring agency at least in every case.

I can summarize this for you quickly here to give you a picture of where we stand on this part of the program.

Task I that I referred to earlier is the underground detection task and the task involves the worldwide seismological network. There is a total of $700,000 available for the procurement of the 50 sets of equipment. I would like to add at this point that this network is only concerned with basic research and development. Data gathering. has nothing to do with the operational detection system. This is strictly an operation for the scientists to get information and that only.

Representative HOLIFIELD. This is for 50 seismological sets of equipment?

Mr. BEYER. Yes, sir.

Representative HOLIFIELD. For the purpose of obtaining statistical background of natural seismic events.

Mr. BEYER. Yes, sir.

Representative HOSMER. There is one other thing on that. Due to the fact that in the existing 50 stations the equipment is different and the procedures are different, this standard calibrated equipment must go in in order that you can accumulate information which comes out on the tape in the same form and shape.

Mr. BEYER. At the present time, as I believe Dr. Romney indicated, the data gathered from many of the seismological stations throughout the world, while it is useful, is not as useful as if you had a standard calibration system and standard instruments.

Representative HOSMER. In making these 50 sets of calibrated equipment, do you factor in the varying capabilities of these seismic stations?

Mr. BEYER. Yes, sir; however, this is under study by this National Research Council Committee as to which types of stations need additional equipment to complete the set they have, which types of stations would have to have almost a complete replacement, because none of their present equipment is amenable to this calibration system. Then there is a third type of category under consideration, which is to fill in the gaps where there is no seismological station. This area exists on the earth and is important to a study of the overall seismic problem.

Representative HOSMER. Are any of these 50 stations in the Iron Curtain countries?

Mr. BEYER. We have not come to any final statement with regard to which stations would be involved and those not. If they are willing to share data with our scientists the same way as they do right now on seismic information, there would be no harm in exchanging equipment with them or providing them with equipment. What we are trying to do is to get as much basic data from useful observation points as we can.

Representative HOLIFIELD. Mr. Beyer, I think we could get this information in the shape that the committee needs it by taking the recommendation of the Berkner report. Do you have it there handy?

Mr. BEYER. No, sir; I do not.

Representative HOLIFIELD. We will furnish you a copy.

Refer to page 15, please. Starting with the first item, equipment of selected stations with modern seismic equipment.

Mr. BEYER. That is the item.

Representative HOLIFIELD. The Berkner report asked for $1,250,000, and you have provided $700,000. Is that right?

Mr. BEYER. That is right.

Representative HOLIFIELD. That is for the first year, of course.

Mr. BEYER. Yes.

Representative HOLIFIELD. And they ask for a million for the second year.

Mr. BEYER. May I add a point here. There is also $50,000 set up to get the proper selection and study made. So if you would care to lump these two together it is $750,000. The funds were made available in late December, so the annual rate would be double this, but the amount for the overall of 1960 would be obviously $750,000.

Representative HOLIFIELD. Let us take the next one, the study of sheer waves, $250,000. How much has been allocated for that?

Mr. BEYER. I think we better have Dr. Romney interpolate between some of these items because they are culled out slightly differently and I think he can help us here. This comes under our task II research program area.

Dr. ROMNEY. Lumping the two items, the study of sheer waves and identification from aftershocks together, these are being conducted as a single study, we have allocated $400,000.

Representative HOLIFIELD. So you have allocated actually $75,000 more there.

Mr. ROMNEY. Yes.

Representative HOLIFIELD. Let us take the next period, long period surface and body waves, that is $1 million for the first year.

Mr. BEYER. The problem we are having, Mr. Chairman, is that the general categories listed in the Berkner report have been further subdivided under these tasks, and as a result we cannot correspond exactly with the dollars, but I think we can for the overall program. We can give you for the record a complete story.

Representative HOLIFIELD. Can you give us for the record a breakdown on the recommendations with corresponding allocations of funds?

Mr. BEYER. I think so; yes, sir. (See attachment 6, p 434.)

Representative HOLIFIELD. Also the amount of contracts which have been let against each specific item and the date of the completion of the contract. If you are negotiating contracts in the field you might just note that you are negotiating. It is of no particular importance to us as to who the contractors are or who you are negotiating with at this time. We only seek to get an overall comprehension of the magnitude of the program in relation to the program advocated, and also the breakdown as to the different sections so we know what areas you are exploring and what areas have not yet been covered.

Mr. BEYER. Yes, sir. May I point out that in this basic research area under the funding in fiscal 1960, $2,350,000 is available. The amount obligated at this time is $2,258,000.

Representative HOLIFIELD. So you have obligated practically all that is available.

Mr. BEYER. Yes; in this area.

Representative HOLIFIELD. That is against the item of $6,575,000 that has been recommended. You have probably not set up your budget for the second year yet, have you?

Mr. BEYER. May I add one comment to your comment. Some of the other items under the detection program which would be part of this Berkner item that you referred to are culled out separately in our program. I can give you the grand total for all types of research.

Representative HOLIFIELD. Would that come as against the recommendation of $22,825,000?

Mr. BEYER. Yes, sir; it comes under that category.

Representative HOLIFIELD. That is the final total?

Mr. BEYER. Yes; it comes under that. It would fall under your $6,575,000 part of it.

Representative HOLIFIELD. You say it would fall under that?

Mr. BEYER. Yes. May I give you then the detection part and the basic research part together, which somewhat corresponds with this $6,500,000 figure. We have $3,600,000 available there and obligated we have $3,492,000.

Representative HOLIFIELD. Are you comparing that to the $6 million figure?

Mr. BEYER. Roughly, yes. That is for 6 months work.

Representative HOLIFIELD. Then, as I understand it, you have a total of $3,492,000, plus $2,350,000 allocated against the $6,575,000 recommendation of the first year.

Mr. BEYER. No, sir. I was giving you too fine a breakdown. This is my fault. If we do consider the total research program——

Representative HOLIFIELD. What do you say? We have two items here. We have individual research projects and we have system development.

Mr. BEYER. That comes later.

Representative HOLIFIELD. And we have nuclear and detonations. You have a total of $22,825,000. Tell me where this $3,492,000 offsets.

Mr. BEYER. All right, sir. The $3,492,000 corresponds to the general category covered by the $6,575,000 area.

Representative HOLIFIELD. $2,350,000 is part of the $3,492,000.

Mr. BEYER. Yes, sir. If we go to the system development area, this was our task III, under our current program, we have on this item $930,000, of which $925,000 has been obligated.

Representative HOLIFIELD. You are now speaking of an offset to $16,250,000?

Mr. BEYER. This corresponds to the $4,250,000 item in the left column.

Representative HOLIFIELD. Give me that figure again.

Mr. BEYER. Available $930,000 and obligated is $925,000.

Representative HOLIFIELD. When were these funds made available to you?

Mr. BEYER. They were made available to us in mid-December of last year.

Representative HOLIFIELD. So you have only had about 3 or 4 months?

Mr. BEYER. They were made available to AFTAC about the 4th of February 1960.

Representative HOLIFIELD. AFTAC allocates funds under your direction?

Mr. BEYER. Yes, sir.

Representative HOLIFIELD. On the $12 million item?

Mr. BEYER. This one refers to the program of nuclear detonations and high explosive detonations, and the fiscal 1960 program provided for funds for two shots. One of them was a high explosive shot. The other was a nuclear shot. We do not have authority to make the nuclear shot at this time. That is that $2,310,000 figure that is available for this part of the program.

Representative HOLIFIELD. $2,310,000 is available.

Has any of that been spent in high explosive tests?

Mr. BEYER. Roughly we have spent $550,000 for equipment which would be used to make the necessary measurements which are a part of the DOD responsibility. If I may, I would like to talk a bit about how these costs are shared. I understand an AEC representative will discuss this later in this program.

Representative HOLIFIELD. The Cowboy shots were handled by AEC funds.

Mr. BEYER. Yes, sir. Then following this part of the program we diverted from the systems cost $195,000, plus some other reserve that ARPA had to pay for these four quick studies that were referred to, and one of which was mentioned yesterday.

Representative HOLIFIELD. A quick computation on my part gives me an obligation of $4,967,000 against an available $7.5 million.

Mr. BEYER. Yes.

Representative HOLIFIELD. Does that sound about right?

Mr. BEYER. That is about right, sir. But it is further complicated, I am sorry to say, because we received in ARPA a total of $8,645,000, and some of these other funds have gone into high altitude studies, into quick studies apart from these four that I mentioned. So we

can give you for the record a complete story of where we stand on this program. (See attachment 6, p. 434.)

Representative HOLIFIELD. That will be fine.

Mr. BEYER. It is very complex, I am sorry to say.

Representative HOLIFIELD. That will include the completion date for the studies, and so forth, I suppose.

Mr. BEYER. Yes, sir.

Representative HOLIFIELD. There are no line items in the physical 1961 budget, are there?

Mr. BEYER. That is correct. In the ARPA submission to Congress it was prepared too late. I am sorry. The budget submission was prepared prior to the assignment of the project to ARPA so it is not included in the congressional submission by ARPA through the Department of Defense.

Representative HOLIFIELD. So this will have to be absorbed out of the bulk research and development moneys of the Department of Defense.

Mr. BEYER. On this point I cannot speak because it is under active consideration right now. In fact, today in the office they are putting together a statement for consideration by top members of the Department of Defense and possibly the principals will discuss this question also.

Representative HOLIFIELD. One of the reasons I am questioning you on this is that we had thought about putting a line item in our authorization bill which we have just voted on in the committee so the project would have a little more importance attached to it. It would look important enough to Congress to ask for a line item on the matter.

Mr. BEYER. Yes, sir.

Representative HOLIFIELD. I am wondering what the future plans are. We do not have the Defense Department budget, of course. I think a line item more properly should be in the defense budget rather than in the AEC budget under ordinary areas of activity. I wonder why this was assigned to ARPA. What was the thinking back of assigning this to ARPA?

Mr. BEYER. I think one of the reasons is that ARPA works in very close contact with Dr. York. It is part of his staff. It has an opportunity to work closely with other Government agencies at top level and to work with all three services.

In addition, it has been able to move rather quickly on previous programs. I think a number of the space programs were started in ARPA when that was part of our general mission.

Representative HOLIFIELD. They have been taken away from ARPA?

Mr. BEYER. They have been transferred.

Representative HOLIFIELD. To NASA?

Mr. BEYER. In part. Partly to the Air Force. The general concept of operation of ARPA is to get a program started and then to move it out as soon as it is well in hand, except for programs which go across the board in all three services. One example of this is the solid propellent basic research project known as Principia. It is of interest to all three services and it provides a means of focusing attention in one area in the Department on this type of basic research problem.

Representative HOLIFIELD. I am surprised that NASA has not absorbed this from you in view of its ability to attract projects in the scientific field, and also in view of its budget, better than $950 million, which was granted.

Mr. BEYER. I was responsible initially for the Saturn program before it was transferred and we were pleased to see it go because it now has a great deal of financial support which could not be made available out of the defense budget.

Representative HOLIFIELD. Would this different placement be an indication that if this project was transferred to NASA it might get a little more attention?

Mr. BEYER. I don't think so, necessarily, because we have preliminary information that indicates that the Defense Department will adequately support this project and has already taken active steps to get it started. That is not a firm commitment because the final answers are being worked out right now.

Representative DURHAM. Was this decision made at the policy level?

Mr. BEYER. Which one, sir?

Representative DURHAM. To continue this program that you have outlined.

Mr. BEYER. The Vela Hotel area policy decision, which is the satellite part of it, has not yet been made. The report is under review at the present time. My understanding is that the Vela Uniform portion definitely will be funded, and that the Vela Sierra, or surface based, high altitude detector program will definitely be funded.

Representative HOLIFIELD. I remember the days when the NASA was operating on about five or six words in the appropriation bill. We pulled that out and gave it importance by authorizing that agency. It looks to me that the Department of Defense, or somebody down there, ought to pull this together and get it on a line item basis as soon as possible. It is a poor way to finance an important project in my opinion after some 18 years experience around here. There are always loose ends and you always have to go to somebody to beg for money, and it takes a lot of time. It is bound to take up a lot of time for somebody, conferring all the time and so on. We are in this field and I think we are in it definitely for a long time to come.

Mr. BEYER. I think I cannot speak as to what method.

Representative HOLIFIELD. I am not asking you to speak. I am just expressing my own opinion.

Mr. BEYER. Thank you.

Representative HOLIFIELD. I have had experience on both sides, the Defense Department and here too. You should put it in one place or another and get a line item for the project.

If there are no further questions, Mr. Beyer, we will excuse you at this time. Thank you very much.

Mr. BEYER. Thank you, sir.

Representative HOLIFIELD. We will expect those materials that you promised us as quickly as possible, so we can go to print.

Mr. BEYER. All right, sir. (See attachment 6, p. 434.)

(The prepared statements of Mr. Beyer follow:)

STATEMENT OF CARLTON M. BEYER, ARPA, "GENERAL RESEARCH PROGRAM REQUIREMENTS FOR ANY AGREED-ON PERIOD OF COOPERATIVE RESEARCH"

I have been asked to discuss the requirements for a program of cooperative research. Such a program was mentioned as a coordinated research program in the statement recently issued by President Eisenhower and Prime Minister Macmillan. The words "coordinated research" are being used to describe a concept that has been proposed under which each participating nation would let the others know of its plans and experiments, share data from these experiments, and compare results obtained from them. Under a coordinated program as currently envisaged, one nation would not approve another nation's plans, experiments or resulting reports. Each participating nation would keep the other informed of all tests and make it possible for others to observe and participate in such tests. The coordination between participating nations could be accomplished by an Information Exchange Group made up of technical and scientific personnel who might report to the Control Commission. This approach would have to be considered and approved at Geneva.

Joint research tasks are not excluded from the concept of a coordinated research program. However, it should be noted that joint efforts would require the approval of the participants with the result that the administrative procedures would probably be more time consuming than those associated with a coordinated program.

Underground detection improvement is receiving active consideration under the coordinated approach. There is no comparable program being considered for the detection of nuclear detonations on the surface or in the atmosphere because the detectability of these blasts at present is considered fairly satisfactory. However, some additional research and development effort will be required before an adequate capability of detection of nuclear detonations in space can be assured. The problem of detecting high altitude nuclear detonations can be separated into two parts: the first involves the use of detection instruments located on the surface of the earth, and the second involves the use of space based or satelliteborne detectors.

Now that the various elements of the coordinated program have been mentioned, I would like to comment briefly on how the current U.S. research and development program is being undertaken. Two developmental approaches are being pursued concurrently. One approach consists of methodical investigation across broad technical fields with the objective of yielding results in the nature of major technological breakthroughs over a period of years. The other approach consists of immediate acceleration and emphasis of work on selected problem areas and is designed to yield many significant improvements of a specialized nature within the next year or two. Under the latter approach, duplicate effort would be placed on areas in which early improvements are most likely.

In undertaking a research and development program involving underground and high altitude detection, it is essential to consider the type and availability of the technical talent that can assist in making rapid progress. Use is being made of many highly qualified scientists and engineers in university, industry, and Government organizations. Increased use will be made of many other experts and specialists in related technical fields.

I would now like to summarize Project Vela, which is the current U.S. program of research and development being undertaken to improve the detection of nuclear detonations occurring under the ground and in space. The basic responsibility for this program resides in the Department of Defense, and is being conducted in cooperation with the Atomic Energy Commission, the Departments of Commerce and Interior, and the National Aeronautics and Space Administration.

Within the Department of Defense, Project Vela was assigned to the Advanced Research Projects Agency on September 2, 1959. The project is divided into three parts, and the following names have been assigned:

"Vela Uniform" for the detection of underground detonations.

"Vela Sierra" for surface based detection of high altitude detonations.

"Vela Hotel" for space based detection of high altitude detonations.

VELA UNIFORM

The first task is being conducted by the Coast and Geodetic Survey and is intended to provide a source of standardized quantitative seismic data on a worldwide basis. This will be accomplished by providing a number of the world's

seismological research stations with standard, calibrated instruments, to be maintained and operated by them as part of their normal routine. The data accumulated at these stations would be exchanged freely among seismologists throughout the world. To accomplish the first step in this endeavor, the National Academy of Science has established a committee of eminent seismologists, who are currently preparing a report recommending the specifications for the standard equipment. The U.S. Coast and Geodetic Survey of the Department of Commerce will procure this equipment and arrange for its distribution. Procurement and distribution is expected to commence in midyear for the first 50 sets of equipment.

Task 2 entails the establishment of a large scale basic research program on the generation and propagation of seismic waves. This is deliberately intended as a stimulation of basic research in the area of seismology, with eventual long-term benefits to the detection problem. Task 2 also consists of a project of a somewhat more applied nature. This aspect involves the investigation of new types of seismic instruments as well as their use in complex arrangements and in locations such as deep in the earth's crust.

Under task 3, in order to make an immediate attack on the requirements for and the detailed characteristics of a system of stations intended for the detection of underground nuclear detonations, it is planned to build and operate two prototype detection stations: one based on the equipment recommended by the Geneva conference of experts, the other containing the improvements recommended by the Berkner panel. Although these stations are expected to serve as prototypes for a final system, they will also be used as the stations for the trial application of new techniques resulting from more basic elements of the program, and for the study and application of refined system techniques such as data processing and analysis.

In task 4, a series of underground nuclear and high explosive detonations have been recommended to provide research data on underground explosions and to test components of a possible control system. These should be conducted in a variety of geological environments, calculated to increase our understanding of the variation in seismic signals resulting from underground weapon tests.

In addition to the long-term program, specific short-term studies are nearing completion on the following subjects:

(*a*) A review of the feasibility of utilizing unmanned seismic stations to extend the capabilities of a detection network.

(*b*) A systematic review of earthquake statistics, and the criteria for differentiating between earthquakes and explosions.

(*c*) A study of the feasibility of utilizing large arrays of seismographs to increase the detection sensitivity of a detection station.

At present the fiscal year 1960 program for Vela Uniform involves $8,535,000 which includes $2,310,000 for planning test shots, if approved.

The entire program is in consonance with the objectives given in the Berkner report. It has been reviewed by the Ad Hoc Group on Seismology formed by Dr. York and more recently by the Ad Hoc Panel on Seismic Research and Development chaired by Dr. Panofsky.

This most recent review resulted in suggestions for improving the accelerated Vela Uniform program. Plans for implementing these suggestions in fiscal year 1960–61 are under active consideration.

VELA SIERRA

The second part of the Vela project, known as Vela Sierra, consists of research and development aimed at improving the detection of high altitude nuclear detonations using instruments located on the earth's surface.

Several techniques for the detection of high altitude detonations with instruments located on the surface of the earth are potentially available; however, there is a need for additional research and development to develop these into useful components of a detection system. Two of the promising areas involve the use of optical and electromagnetic methods. I will mention these briefly since the technical experts will discuss them in greater detail during these hearings.

Optical methods.—There are two techniques available which depend on the detection of visible light. One measures the light emanating directly from the explosion. The other measures the fluorescence or glow of the upper atmosphere which results when the radiation from the nuclear detonation interacts with atmospheric molecules. Although the basic phenomena which occur in each case are relatively well known, the effects of the natural phenomena must be better understood before detection instruments can be developed which are

reliable enough to be used in a control system. Specifically, lightning flashes, the aurora, and the presence of the clouds which obscure the view of the instruments, will adversely affect their performance.

Electromagnetic methods.—The detonation of a nuclear weapon is accompanied by the emission of a radio signal. Detection of this signal by a suitable radio receiver appears to be an attractive possibility. Before this signal can be used, two things must be better understood: First, there has to be a better understanding of the frequency and time characteristics of the emitted signals; secondly, the natural atmospheric noise generated by lightning storms has close similarity to the signal that is to be detected, and methods for discriminating between the detonation signal and the natural background must be devised.

The propagation of radio signals between points on the surface of the earth or from outer space is significantly affected by the ionized regions of the upper atmosphere called the ionosphere. After a nuclear detonation has taken place, radio signals may be absorbed, the direction of propagation may be changed, and the signal may be scattered, depending on the degree of ionization, its distribution in space, and the frequency of the radio waves. In principle, these modifications to radio propagation conditions can be detected, and used as an indication that a nuclear detonation had occurred; however, natural perturbations also occur in the atmosphere. These natural perturbations result from solar flares, meteors and auroral activity. Before this approach can be used, it will be necessary to understand more about natural variations in the ionosphere and radio signal propagation conditions.

With the examples that I have discussed very briefly I have tried to show several of the approaches being considered under the Vela Sierra program which is just getting underway. Funds for commencing this work were received 2 weeks ago and are in the amount of $1,050,000. Additional work is planned.

VELA HOTEL

The third part of the Vela project is known as Vela Hotel. This portion of the project consists of research and development associated with space-based techniques for detecting nuclear detonations which occur in outer space beyond the effective range of the surface based detectors just discussed under Vela Sierra. The method most likely to be employed requires the detection of signals generated by detonations in space with special instruments mounted on satellites, and then relaying the information to ground stations.

At present, the use of X-ray, gamma-ray, and neutron detectors appears to offer the best approach for early development of a practical operational system. The ability to detect a nuclear detonation by this method is dependent on at least the following factors:

(*a*) The amount of radiation emitted from a nuclear detonation.

(*b*) The distance between the detonation and the detection system.

(*c*) The natural radiation background or environment in which the detection system is operating.

(*d*) The sensitivity of the detection system, sensors, and associated electronic instrumentation.

(*e*) Any shielding or other countermeasures which might be employed to conceal the nuclear detonation.

In order to be able to build and operate an effective space-based detection system, it will be necessary to investigate each of these factors. A research and development program oriented to accomplish this would include:

(*a*) Study and examination of the basic mechanisms of nuclear weapon detonations in space. This will result in a refinement of our understanding of the detectable radiations emitted and may lead to the discovery of new and more effective detection methods.

(*b*) Determination of the natural radiation background in space where satellite-borne detection systems are to be located.

(*c*) A large effort on the improvement of X-ray, gamma-ray, and neutron sensing devices with particular emphasis on advanced techniques.

(*d*) In conjunction with the work on detectors, considerable effort on the electronic equipment which analyzes the signals detected. This signal analyzer, or logics as it is sometimes called, provides the capability of discriminating against natural background signals.

(*e*) Research on promising detection schemes other than those derived from sensing X-ray, gamma-ray and neutron signals. Included in the approaches to be investigated are the detection of: air fluorescence, direct visible light, other electromagnetic signals such as radio waves, and light scattered by the bomb

debris. The objective of this work would be to devise improved techniques which would lead to more effective detection capability.

(*f*) Thorough investigation of all aspects of equipment reliability. The objective is to increase the useful operating time of the equipment. This will tend to reduce program costs since the required testing and acquisition of data can be achieved with fewer replacement parts and in less overall time.

(*g*) Study and research for countering any techniques for concealment which might be utilized to mask a nuclear detonation from a surveillance system.

I have made no reference to the space vehicles required to place the detection payload into orbit. Vehicles developed under existing programs appear to be suitable. The information regarding Vela Hotel which I have just discussed was derived from a report recently prepared by a technical working group established by ARPA. Participating on the working group were technical personnel from the USAF Ballistic Missile Division, Atomic Energy Commission, National Aeronautics and Space Administration, and associated contractors. The coordinated report was completed roughly 2 weeks ago and is now undergoing review by the agencies involved.

A decision with regard to action to be taken on this program will be made in the near future. In the meantime, the Department of Defense has initiated the necessary planning in conjunction with the AEC and NASA to flight test appropriate experimental gear as piggybacks on forthcoming DOD and NASA space probe and satellite flights.

SUMMARY

In summary, I would like to say that ARPA, in the Department of Defense, is vigorously prosecuting the Vela project in conjunction with the AEC, NASA, the Departments of Interior and Commerce, and with substantial assistance from universities and industry. The Vela Uniform program is well underway. The Vela Sierra program is funded and just getting started. A comprehensive study has been made of the Vela Hotel problem and the resulting report is being actively reviewed by top level Department of Defense, Atomic Energy Commission, and National Aeronautics and Space Administration personnel.

STATEMENT OF CARLTON M. BEYER, ARPA

Subject: Discussion of Geneva-type International Control System now possible under present state of knowledge—Requirements (dollars and manpower) for the underground and space systems.

I have been asked to speak on the requirements in terms of dollars and manpower for the underground and space systems of a Geneva-type International Control System.

The ARPA responsibility that was assigned by the Secretary of Defense and the Director of Defense Research and Engineering is for research and development to improve our ability to detect nuclear detonations underground and in space. ARPA is not responsible for the construction, staffing, or operation of a control system, but only for the underground and space research and development aspects.

For assistance in this hearing, estimates have been obtained from previously prepared studies that reflect the general magnitude of the cost and effort required to establish a control system for detecting nuclear detonations underground, in the atmosphere and at high altitude. These estimates are likely to be affected by further negotiations at Geneva and to the cost of R. & D. on space satellites or the use of space satellites, nor did they include R. & D. on underground detection. The estimates presented may be reduced if detection technique breakthroughs are achieved as a result of the research and development under Project Vela in the Department of Defense, or they may be increased if techniques for concealing nuclear detonations are improved.

It should also be understood that since a number of details regarding the control system have not been agreed on at Geneva, the estimates cannot be precise and are likely to be changed significantly as a result of further negotiation.

In order to install and operate a worldwide control system quickly and effectively, the proper detection equipment, an efficient organization with excellent interunit communications, and clear, previously agreed-upon rules and procedures are required.

If implemented the overall responsibility for the control system would be exercised through a multination control commission to which the Administrator for the Control System would report. He would be located at system headquarters.

Headquarters.—The system would be controlled from headquarters where a data analysis center, a research office, and a central laboratory would be located. Also several staff offices would be required.

Regional offices.—Regional or intermediate offices are envisioned for the administrative supervision and logistic support of control posts.

Control posts.—In areas subject to few earthquakes, control posts would be spaced at 1,700-kilometer intervals.

Where there is considerable seismic activity, the control post spacing should be about 1,000 kilometers. In ocean areas, the ship and island control posts would be spaced between 2,000 and 3,500 kilometers apart.

	Number of control posts
On continents	110
On large oceanic islands	20
On small oceanic islands	40
On ocean vessels	10
Total	180

The techniques employed in a control post consist primarily of sensing devices or instruments that record disturbances of the earth or atmosphere. The recordings of several techniques and several control posts are analyzed and compared, to determine whether a disturbance was natural or man-made. Such a determination is not always possible, particularly if the disturbance is of low magnitude or low yield. To assist in this determination, inspection teams will be used to investigate sites of suspected nuclear explosions. The inspection teams should be located at selected regional offices and land control posts in such a way that the inspection teams would have prompt access to suspected sites and should be equipped to complete the inspection. The number and makeup of the teams are still under discussion at Geneva. Aircraft are to be used that are equipped with devices for taking samples of atmospheric debris. (Similar devices are also located at each control post.) These samples are then analyzed to determine if they include any nuclear debris and, if so, if such nuclear debris is of recent origin. The system described so far does not provide for detection of nuclear detonations in R. & D. for space detection, particularly the use of satellites, is being studied by ARPA. Also, further research is in progress on underground detection. I will cover these points in more detail later.

Communications system.—Rapid and reliable communications will be required to transmit data messages to the Headquarters. These requirements will probably necessitate the use of radio.

Personnel.—It appears that a limited number of technically trained personnel is available for many of the positions in the system. An extensive training program would be required. Since a number of nations would be involved in the control system, all rules, procedures and communications would have to be designed so that the several languages would pose no barrier.

It is estimated that approximately 20,000 people may be required to establish and operate the system, excluding space detection. The great majority of these people will be required in the control posts as technicians and as support personnel. A control post would have approximately 30 to 35 technicians with number of additional support personnel that would depend on the remoteness of the post from support resources. The system headquarters may require 1,500 to 2,000 personnel, the majority to be employed in the analysis of data submitted by the control posts. Other personnel requirements will be in the fields of air operations, regional offices, ships and communications.

Cost.—It is estimated that the International Control System, without satellites, will cost the participating governments in the neighborhood of $1 billion to establish and $250 million a year to operate. It would cost the United States from one to several million dollars to establish a control post depending upon the climate and support resources available locally. The final siting of a control post will of course determine the cost. The final site cannot be determined until detailed map studies and visual and instrument surveys have been made and the results analyzed. In the establishment of a control post, we must consider such things as building materials, construction, fuel storage, heating facilities, pole lines, electric power, water wells, communications, technical equipment, housing, dining facilities, transportation, initial supply and resupply, and roads.

In some cases, control posts will have to be completely self-sufficient. The annual operating costs will consist largely of salaries but will cover also such items as resupply of technical and other equipment and supplies, transportation, research and development and training.

Space-based detection system.—I can only speak in general terms about the space-based operational detection system, since the detection capability requirements have not yet been established for this environment. The detection effectiveness of satellite-borne detectors is a function of the instrumentation's sensitivity and ability to discriminate between signals produced by a nuclear detonation and by natural background phenomena. It is possible to obtain various limited levels of detection capability over the next few years. However, by conducting necessary research and development, detection effectiveness can be rapidly improved with time.

Surface-based high-altitude detection system.—A capability to detect nuclear detonations to a limited distance from the earth can be achieved by using detection instruments located on the ground. These instruments would be located at the control posts.

Underground.—The resource estimates I have indicated and the system I have described includes undergound detection techniques based on the present state of the art.

In summary.—Approximately 20,000 people would be required to operate a control system capable of detecting underground, surface, and atmospheric detonations. The cost of establishing the system would be approximately $1 billion, take about 6 years to establish, and the annual operating cost would be roughly $250 million. All these costs would be shared by the participating countries. These estimates specifically exclude outer space detection that requires the use of satellites.

Representative HOLIFIELD. I am going to try to expedite the rest of the afternoon. I understand some of you gentlemen have to catch planes. So all of the witnesses come to the table. Our next witness is Dr. Richard Latter of the Rand Corp., who recently has been appointed to the Ad Hoc Group of Seismic and High Altitude Detection to the Department of Defense.

Dr. Latter will testify on the needs for explosions in the research program. A later witness, Dr. Gerald Johnson, of the Lawrence Radiation Laboratory at Livermore, will discuss nuclear explosions for research.

Dr. Romney was to follow Dr. Latter, but has another meeting to attend and has asked to be excused. You may proceed, Dr. Latter.

Dr. LATTER. Yes, sir. There is a slight point. I will also talk about the need of chemical versus nuclear.

Representative HOLIFIELD. Dr. Romney, we will excuse you if you are needed at another meeting.

Dr. ROMNEY. I would appreciate it. I have prepared a statement which could be submitted for the committee (p. 396).

Representative HOLIFIELD. We certainly appreciate your testimony, Dr. Romney. You submit that to the staff and we will include it in the record and forgo the opportunity of questioning you.

Dr. ROMNEY. Thank you.

Representative HOSMER. Before he gets away I would like to ask if he or any of the other witnesses in light of all this testimony regarding the necessity for a research program and the amount of money that would have to go into the project, and so forth, all of this presupposes that even if you could instantaneously manufacture and place in operation a detection system based on present knowledge, it would not be a detection system so far as underground or space shots are concerned that would deter cheating. Does anybody disagree with that?

Dr. ROMNEY. No.

Dr. JOHNSON. No.

Dr. LATTER. No.

Representative HOLIFIELD. Before you leave, Dr. Romney, do you feel that your seismological program, the part that you are responsible for, should have a more aggressive and vigorous support?

Dr. ROMNEY. Sir, it has gotten off to a good start now. It is going about as fast as we can handle it at the present time. However, it has been recommended that if we wish to accomplish specific objectives in a shorter time the program could use additional money.

Representative HOLIFIELD. You have submitted those recommendations?

Dr. ROMNEY. We have worked with ARPA on preparing this kind of a recommendation; yes.

Representative HOLIFIELD. I wonder if we could have copies of your submission on that point. Would that be a matter of contacting ARPA?

Dr. ROMNEY. I believe it should come through ARPA since they are responsible for the budget.

Representative HOLIFIELD. We will, at a later date, make that request of Mr. Beyer.

Representative DURHAM. In making that statement, Doctor, does that mean a requirement of more funds or requirement of more seismologists and scientific personnel? Which is the most important?

Dr. ROMNEY. There certainly is a limited number of people trained in seismology at the present time. However, there are related scientific groups in industry who could participate actively in the program if we solicited their help. The oil industry, for example, through their prospecting efforts have a manpower pool of talent which could be tapped and has not been tapped.

Representative DURHAM. Those are primarily the people who would make the instruments.

Dr. ROMNEY. Instruments, and also theoretical people.

Representative DURHAM. Thank you very much.

Representative HOLIFIELD. The Chair is going to reserve the privilege of scrutinizing these various statements and including particularly Dr. Romney's statement which we have not heard, with the idea in mind of possible letters for either clarification or expansion of points that are brought up. The Chair wishes to say again that the witnesses that have been before us will have the opportunity, as I promised them, of submitting additional material for clarification or even material which they think would be of benefit to the committee on any of the points brought up during the testimony.

(The statement of Dr. Carl F. Romney follows:)

SEISMIC MEASUREMENTS OF LARGE EXPLOSIONS

(By Dr. Carl F. Romney, Air Force Technical Application Center)

I. THE NEED FOR AN EXPLOSION PROGRAM

The present difficulties in detecting and identifying underground nuclear explosions can best be overcome by finding, in the seismic waves produced by explosions, some characteristic which is not produced by any natural earthquake. If such a characteristic signal could be found, and the seismographs of the control system recorded this signal, inspectors could be dispatched to the scene to see if the explosion was produced by a nuclear reaction. To date no such characteristic signal has been found.

Our present state of knowledge requires that all seismic disturbances be treated as potentially suspicious occurrences unless, through a process of careful study, we are able to identify certain of these disturbances as having originated from

natural causes. A few characteristics are known which can be used to make such an interpretation. For example, if it can be shown that the point of origin was deeper than can be reached by drilling techniques or if it can be shown that the earth initially moved inward toward the source, then there would be a high degree of confidence that the event was a natural earthquake. However, except for very large disturbances, most of the natural seismic events will remain in an ambiguous "unidentified" category.

Onsite inspection has been suggested as the means for further characterizing events in the unidentified category. It is doubtful, however, that small earthquakes in general produce effects which can be interpreted by onsite inspection as unambiguous evidence that the source of the disturbance is, in fact, a natural earthquake. Therefore the class of unidentified events will seldom be reduced in size by inspection except in those cases where the inspectors are able to find proof that an explosion has occurred. A typical onsite inspection can be expected to result only in the conclusion that it has not been proved that an explosion occurred. The degree of confidence with which it could be stated that the questionable event was probably natural will depend critically on the vigor of the inspectors and the thoroughness of their investigation.

The most effective way to improve the capability of the control system is to find means to reduce the number of events which require inspection. Thus the development of criteria for characterizing explosions, as well as the development of additional criteria for characterizing earthquakes, is fundamental to the nuclear weapons control problem. In view of the present status, it is clear that additional measurements of seismic waves generated by large explosions will be required.

II. RESEARCH OBJECTIVES

Our ability to predict the capabilities of proposed systems for seismic detection and identification of underground nuclear tests, and to properly develop and evaluate improvements of such systems, is dependent upon a significant increase in our understanding of the seismic motions at all distances from underground nuclear explosions. The "Report of the Panel on Seismic Improvement," chaired by Dr. Lloyd V. Berkner, included the following:

"Our knowledge of seismic wave generation by large explosions, particularly nuclear explosions, is very limited. Only three completely contained underground explosions with yields greater than 1 kiloton have been fired, all under very similar environmental conditions. The parameters that can significantly affect the magnitude and type of seismic effects produced by nuclear explosions should be experimentally explored so that theories can be developed which will permit reasonable confidence in our understanding of these parameters and deductions about them. The following parameters require study:

"(*a*) Dependence of spectra of body and surface waves on yield of the explosion.

"(*b*) Dependence of seismic wave excitation on the medium surrounding the shot.

"(*c*) Effect of depth of burial.

"(*d*) Effect of local environment such as shot-cavity size and shape.

"(*e*) Effect of local geology and topography."

In addition to those studies quoted above, further investigations should be made of:

(*a*) Variations with distance of the amplitudes of the several types of seismic waves.

(*b*) Variations in the size and frequency of seismic waves due to the type of explosive used, i.e., nuclear versus chemical explosives.

III. MEASUREMENT PROGRAM

1. The primary seismic data for use in developing improved detection and identification methods will be derived principally from temporary observatories established at locations determined by the specific nature, location and size of the shot. It is necessary to establish temporary observatories because the existing seismological observatories will usually not be located at the desired distances and directions from the explosion. In addition, few of the existing stations are equipped with instrumentation which is adequate in frequency response or in precision of calibration to meet the requirements of the research program. At some future time, more reliable and useful data of a supplementary nature will be recorded at the reequipped standard observatories of the world. For the foreseeable future, however, a typical measurement program will consist of the following field programs:

(*a*) *Close-in measurements*

The accelerations, velocities, strains, and pressures caused by an explosion will be measured with instruments placed in boreholes above, below and horizontally surrounding the shot, at distances from a few feet to a few hundred feet. The resulting measurements will be used to test theoretical predictions regarding the basic explosive phenomenology. When suitable theories have been developed, and proved, it will be possible to predict effects at other yields and in other media, thus reducing the number of shots required.

(*b*) *Intermediate-range measurements*

At distances from about 1,000 feet to perhaps 50 miles, along the earth's surface, strong-motion acceleration and velocity measurements will be made in the frequency range below 100 cycles per second. The resulting data will be used to measure total energy input into seismic waves and to test theories on waveform distortion over relatively simple paths. About six strong-motion stations and six sensitive seismograph stations will be required.

(*c*) *Long-range measurements*

Twenty stations will be established at distances beyond 50 miles and extending to 2,000 or 3,000 miles. Generally, the stations will be equipped with both short-period (about 1-second) and long-period (greater than 10 seconds) seismographs. As time and funds permit, recording systems will be converted from photographic to tape storage units to simplify performing more complex analyses. The majority of the improvements in detection will result from this data.

2. In addition to the measurements discussed above, which are directed primarily toward fulfilling fundamental research requirements, operational tests of detection stations and equipment should be conducted. These programs include:

(*a*) *Test of conference of experts network*

A system should be established consisting of perhaps six stations employing all of the equipment recommended by the Geneva conference of experts. In addition to providing basic research data of high quality, the stations will provide a definitive test of the capability of a control system constructed according to the recommendations of the conference of experts.

(*b*) *Test of unmanned stations*

The utility of simple, unmanned stations can be investigated through and operational test involving small (10–30 tons) explosions, although valuable data may also be obtained from larger explosions. Such stations have been suggested as a means to supplement the control system recommended by the conference of experts.

(*c*) *Test of manned simplified control stations*

Supplementary stations, perhaps consisting of all equipment recommended by the Conference of Experts except the arrays of short-period seismographs, should be tested.

(*d*) *Test of new devices and techniques*

Improved seismographs, larger arrays, deep-hole detectors, etc., as well as improved analytical techniques for determining the nature of a seismic source should be operationally tested as soon as they are available.

IV. SCHEDULE AND COSTS

A field measurement program of the size and complexity discussed in paragraph III. 1., above, could commence about September 1960. In most cases, it would be desirable to wait about 3 months between major explosions to allow time for the evaluation of data, reevaluation of plans for future explosions, selection of sites for observatories and installation of detection equipment. However, certain series of explosions, for example, a sequence of explosions primarily to investigate decoupling, could be conducted with relatively short time intervals between shots. The field measurement programs will cost about $500,000 to $1 million per explosion, exclusive of costs for construction, emplacement, and the explosive itself.

The operational evaluation of control stations and detection systems as discussed in paragraph III. 2, above, could not be completed in less than 18 months to 2 years, primarily because of the time required for constructing the seismographic stations. Costs cannot be estimated with precision at the present time because certain detection equipment and auxiliary equipment has yet to be developed. However, it can be expected that the costs for a reasonably complete program will

exceed $10 million in 2 years. About one-third of this cost will be for the construction and evaluation of the detection performance of model control stations, and the remaining two-thirds will be for field measurements.

Representative HOLIFIELD. So you may proceed, Dr. Latter.

STATEMENT OF DR. RICHARD LATTER,[1] THE RAND CORP.

Dr. LATTER. Thank you, sir.

I would like to discuss the questions which can be answered only by a research program using underground explosions. I will also comment on the specific need for nuclear as opposed to chemical explosions.

The basic problem of a control system is to detect and locate seismic events and to distinguish natural earthquakes from underground nuclear explosions. To do this job we need to have extensive knowledge about seismic signals from nuclear explosions. We need to know about explosions carried out under the widest variety of conditions, including deliberate attempts at concealment.

But we have, in fact, only very limited knowledge. All we know about seismic signals from underground nuclear explosions we learned from the Rainier shot and the five Hardtack explosions. All these explosions were limited to a unique set of conditions and to a single environment.

Representative HOLIFIELD. Would it be a fair question to ask you if you are precluded from having nuclear explosions to test the seismograph readings in different types of underground material, that there would be a large area where there would be a vacuum, you might say, in the seismological knowledge?

Dr. LATTER. I will try to answer that in considerable detail a little later in my report.

To be more specific, one of our most urgent needs is to learn more about the peak motion and first motion amplitudes produced by underground nuclear explosions. Peak motions are important because they are the basis for detection. First motion is important because it is our own criterion for distinguishing explosions and earthquakes.

The Hardtack data are completely inadequate to determine the peak signal and first motion amplitudes in the far zone where, in fact, most control stations will be located. There is no way to obtain this important information without the use of underground explosions.

Another of our urgent needs involves the so-called diagnostic aids. From these must come most of the improvements in the system. One of the most hopeful of these aids involves the use of long-period surface waves to deduce the cause of a seismic signal. In principle this could be a very effective method. However, we have had a previous experience with surface waves and we are now more cautious. The so-called surface love ways were conjectured to be characteristic only of an earthquake, but, in fact, they were also found in the Blanca explosion.

This example shows us the primitive state of this field and emphasizes the need for more experimental explosions. I will not discuss the other aids. There are many more. But each has a large degree of uncertainty and all will require underground explosions.

[1] See biographical sketch, p. 88.

The question naturally arises as to what kinds of explosions are needed. Do they have to be nuclear or will chemical explosions be good enough? My comments here will be primarily technical, although it is my understanding that a program of nuclear explosions can be undertaken in a substantially shorter time, at less cost and probably with greater ease than a program involving chemical explosions of equivalent yield.

From a purely scientific point of view, there can be no doubt that the optimal approach to learning about signals generated by nuclear explosions and to obtaining results directly applicable to the control problem is by the direct use of nuclear explosions themselves.

The reason we are concerned about the difference between chemical and nuclear explosions is, first of all, because they are different. Chemical explosives are enormously larger, both in mass and volume, than the equivalent nuclear charge. Moreover, in nuclear explosions much higher energy densities and temperatures and greatly reduced amounts of explosion products are produced. We are concerned that these differences will lead to different seismic effects.

At the present time we have only three pieces of evidence bearing on the magnitude of the chemical-nuclear difference. First we know that the intensity of chemical explosions in air and in water is greater by a factor of two than for nuclear explosions of the same yield. Similar differences might be expected for explosions occurring underground.

A second bit of evidence is the volume of the cavity produced by tamped underground explosions. It has been estimated from an extrapolation of the Cowboy results that the cavities produced by chemical explosions in a salt medium are two or three times smaller than the equivalent Rainier cavity in Nevada tuff. Unfortunately, this comparison involves both the difference in medium as well as the chemical-nuclear difference.

A third bit of information is obtained by comparing the seismic records from a 50-ton chemical explosion at a depth of 160 feet in Nevada tuff with those from the 70-ton nuclear shot Tamalpais at 330 feet in tuff. This comparison indicates that within experimental uncertainty, the signals from these explosions are very similar. However, considering both the differences in yield and depth of burial, it is probably safe to conclude only that the chemical-nuclear difference for tamped explosions in tuff is less than a factor of two.

The conclusion that one draws from all this evidence that the chemical-nuclear difference is a factor of two or so applies mainly to the strength of the seismic signal. For identification and for the diagnostic aids, however, it is necessary to consider more subtle features of the seismic signal.

It seems possible that for these purposes the chemical-nuclear difference could be much greater. Many of the aids, for example, depend for their usefulness on the degree of symmetry of the explosion. Nuclear explosions are inherently very symmetrical, but it is difficult to assure symmetry in the case of large chemical explosions.

Another difficulty of a more general nature with chemical explosions is the problem of determining the yield of a tamped chemical explosion. If the yield is calibrated by an equivalent chemical explosion in air, there will be an uncertainty about how the kinetics of the reaction are influenced by tamping.

We see, then, that all in all there are a number of uncertainties about using chemical explosions to learn about nuclear explosion signals. The very fact that we are uncertain about the extent of the chemical-nuclear difference is a strong scientific justification for having nuclear explosions.

I would like now to turn to another important problem for the research program, the concealment problem. Experiments on concealment have up to now been conducted exclusively with chemical explosives. While the theoretical expectations for decoupling have been confirmed, there still remain important questions which need to be studied.

One of these is to determine the minimum cavity size which will effectively decouple nuclear explosions. Because the difference between a chemical and nuclear explosion in a cavity is known to be great—known to be great in a cavity—it is likely that this question can be answered satisfactorily only with the aid of nuclear explosions.

Some suggestions that have been made for increasing the decoupling beyond the present well-established factor of 300 depends specifically upon the high energy densities and temperatures associated with nuclear explosions. Furthermore, to study the survival of the cavity after repeated use, which affects the cavity cost, will require nuclear explosions.

Finally, I would like to mention the problems connected with the techniques for onsite inspection. Here again it is likely that results would be less meaningful from chemical explosions. Certainly if radiological methods are studied. Furthermore a lot more human effort is required at the explosion site to set off a chemical explosion. A chemical explosion requires a large cavity to contain the large volume of high explosive needed to give a yield comparable to nuclear explosions. In addition, the transportation and installation of the high explosives require considerably more human and engineering activity.

In summary, I believe that underground explosions will be required to answer many important questions concerning the control system. Some of these explosions may be chemical, but to answer some important questions, nuclear explosions are necessary.

Representative HOLIFIELD. Thank you, sir.

Representative HOSMER. It just boils down in plain commonsense requiring nuclear explosions to find out the effects of nuclear explosions which cannot be extrapolated from some unknown factor or another kind of explosion.

Dr. LATTER. That is essentially right. There are things that you can learn from chemical explosions that will bear on nuclear signals. Scientifically speaking, the best way of learning about nuclear signals is through the direct use of nuclear explosions.

Representative HOLIFIELD. I would like to ask you to enlarge a little bit on your statement that a program of nuclear explosions can be undertaken at less cost than a program of chemical explosions.

I will ask you this question: What would be the cubic size of a mass of high explosive which would be equivalent to a 20-kiloton nuclear bomb, and what is the comparison in cubic size?

Dr. LATTER. Sir, I believe that Dr. Gerald Johnson is going to talk in detail just to your point.

Representative HOLIFIELD. Then we will let Dr. Johnson present his statement and if that question is not answered we will come back to it.

(Dr. Richard Latter's prepared statement follows:)

THE NEED FOR EXPLOSIONS IN THE RESEARCH PROGRAM

(By Richard Latter, the Rand Corp.)

I would like to discuss the questions which can be answered only by a research program using underground explosions. I will also comment on the specific need for nuclear, as opposed to chemical, explosions.

The basic problem of a control system is to detect and locate seismic events, and to distinguish natural earthquakes from underground nuclear explosions. To do this job we need to have extensive knowledge about seismic signals from nuclear explosions. We need to know about explosions carried out under the widest variety of conditions, including deliberate attempts at concealment.

But, we have in fact only very limited knowledge. All we know about seismic signals from underground nuclear explosions we learned from the Rainier shot and the five nuclear explosions of the HARDTACK series. And these explosions were limited to a unique set of conditions and to a single environment.

To be more specific, one of our most urgent needs is to learn more about the peak motion and first motion amplitudes produced by underground nuclear explosions. Peak motion is important because it is the basis for detection. First motion is important because it is our only criterion for distinguishing between explosions and earthquakes. The HARDTACK data are completely inadequate to determine the peak signal and first motion amplitudes in the far zone, where in fact most control stations will be located. There is no way to obtain this important information without the use of underground explosions.

Another of our urgent needs involves the so-called diagnostic aids. From these must come most of the improvements in the system. One of the most hopeful of these aids involves the use of long-period surface waves to deduce the cause of a seismic signal. In principle this could be a very effective method. However, we have had a previous experience with surface waves and we are now more cautious. The so-called surface Love waves were conjectured to be characteristic only of an earthquake, but in fact they were also found on the Blanca explosion. This example shows us the primitive state of this field and emphasizes the need for more experimental explosions. I will not describe the other aids—there are many more—but each has a large degree of uncertainty and all will require underground explosions.

The question naturally arises as to what kinds of explosions are needed. Do they have to be nuclear, or will chemical explosions be good enough? My comments here will be primarily technical, although it is my understanding that a program of nuclear explosions can be undertaken in a substantially shorter time, at less cost and probably with greater ease than a program involving chemical explosions of equivalent yield.

From a purely scientific point of view there can be no doubt that the optimal approach to learning about signals generated by nuclear explosions and to obtaining results directly applicable to the control problem is by the direct use of nuclear explosions themselves. The reason we are concerned about the difference between chemical and nuclear explosions is first of all because they are different. Chemical explosives are enormously larger, both in mass and volume, than the equivalent nuclear charge. Moreover, in nuclear explosions much higher energy densities and temperatures and greatly reduced amounts of explosion products are produced. We are concerned that these differences will lead to different seismic effects.

At the present time we have only three pieces of evidence bearing on the magnitude of the chemical-nuclear difference. First, we know that the intensity of chemical explosions in air and from water is greater by about a factor of 2 than for nuclear shots of the same yield. Similar differences might be expected for explosions occurring underground. A second bit of evidence is the volume of the cavity produced by tamped underground explosions. It has been estimated from an extrapolation of the Cowboy results that the cavities produced by chemical explosions in a salt medium are two or three times smaller than the equivalent Rainier cavity in Nevada tuff. Unfortunately, this comparison involves both the difference in medium as well as the chemical-nuclear difference. A third bit of information is obtained by comparing the seismic records from a 50-ton chemical explosion at a depth of 160 feet in Nevada tuff with those from the 70-ton nuclear shot Tamalpais, at 330 feet in tuff. This comparison indicates

that within experimental uncertainty the signals from these explosions are very similar. However, considering both the differences in yield and depth of burial, it is probably safe to conclude only that the chemical-nuclear difference for tamped explosions in tuff is less than a factor of 2.

The conclusion one draws from all this evidence, that the chemical-nuclear difference is a factor of 2 or so, applies mainly to the strength of the seismic signal. For identification and for the diagnostic aids, however, it is necessary to consider more subtle features of the seismic signal. It seems possible that for these purposes the chemical-nuclear difference could be much greater. Many of the aids, for example, depend for their usefulness on the degree of symmetry of the explosion. Nuclear explosions are inherently very symmetrical, but it is difficult to assure symmetry in the case of large chemical explosions.

Another difficulty of a more general nature with chemical explosions is the problem of determining the yield of a tamped chemical explosion. If the yield is calibrated by an equivalent chemical explosion in air there will be an uncertainty about how the kinetics of the reaction are influenced by tamping.

We see then that all in all there are a number of uncertainties about using chemical explosions to learn about nuclear explosion signals. The very fact that we are uncertain about the extent of the chemical-nuclear difference is a strong scientific justification for having nuclear explosions.

I would like to turn now to another important problem for the research program—the concealment problem. Experiments on concealment have up to now been conducted exclusively with chemical explosives. While the theoretical expectations for decoupling have been confirmed, there still remain important questions which need to be studied. One of these is to determine the minimum cavity size which will effectively decouple nuclear explosions. Because the difference between a chemical and nuclear explosion in a cavity is known to be great, it is likely that this question can be answered satisfactorily only with the aid of nuclear explosions. Some suggestions that have been made for increasing the decoupling beyond the present well established factor of 300 depend specifically upon the high-energy densities and temperatures associated with nuclear explosions. Further, to study the survival of the cavity after repeated use, which affects the cavity cost, will require nuclear explosions.

Finally, I would like to mention the problems connected with the techniques for on-site inspection. Here again it is likely that results would be less meaningful from chemical explosions—certainly if radiological methods are studied. Furthermore, a lot more human effort is required at the explosion site to set off a chemical explosion. A chemical explosion requires a large cavity to contain the large volume of high explosive needed to give a yield comparable to nuclear explosions. In addition the transportation and installation of the high explosive require considerably more human and engineering activity.

In summary, I believe that underground explosions will be required to answer many important questions concerning the control system. Some of these explosions may be chemical but, to obtain answers to some important questions, nuclear explosions are necessary.

Representative HOLIFIELD. We will now hear Dr. Johnson's statement.

STATEMENT OF DR. GERALD W. JOHNSON,[1] LAWRENCE RADIATION LABORATORY, LIVERMORE, CALIF.

Dr. JOHNSON. Mr. Chairman and members of the committee, with your permission, I would like to preface my remarks with respect to the seismic improvement program with a few remarks concerning weapons development testing in outer space.

This is in response to a question you directed earlier today to Dr. Leonard of the Space Technology Laboratory, so with your permission, I would like to answer that question for you.

Representative HOLIFIELD. Go right ahead.

Dr. JOHNSON. In several laboratories, including the Lawrence Radiation Laboratory, during the past 2 years serious study has been devoted to the technical problems of weapons development testing in outer space.

[1] See biographical sketch, p. 222.

As Dr. Leonard pointed out, methods are presently available for placing nuclear devices in space and for transmitting and receiving information from them out to several tens of millions of miles. We at Livermore have examined the possibilities of measurement and feel that the basic measurements necessary to a weapons development program can be obtained with rather small extensions of existing techniques.

It is possible to recognize that the basic principles of most new designs could be developed at energy releases below a few tens of kilotons. Outer space testing would permit the checking of the data at full scale.

So the two programs or two approaches would permit development and testing of weapons over the full range of energy release of possible military interest. Because outer space testing would involve mainly merely a check of more detailed measurements at smaller scale, the requirements for measurements are not as great.

In fact, the principal measurement would be that of total energy release, which we believe would not be difficult. It would perhaps be the most simple quantity to determine. The more difficult measurements involving high accuracies, particularly in time measurements are those of the determination of the characteristics of the reaction itself.

However, even in this case, concrete ideas have appeared which probably will work well enough the first time they are tried. An experiment to check the proposed techniques could be conducted in a well-instrumented way in a relatively short time.

Routine weapons development tests could be started immediately after this first calibration and techniques would improve rapidly. With respect to the possibility of the conduct of a weapons development program in an undetected way, I would like to refer back to Dr. Brown's earlier statement.

As Dr. Brown testified, and this will not quite be verbatim, but very close to what he stated: "If a violator can cheat with explosions up to 1 kiloton, he can do all the work necessary to the development of small tactical weapons. If he can cheat to 20 to 30 kilotons he can do a lot, but not all he would probably want to do in developing strategic weapons."

If, in addition, he can cheat in outer space in the megaton range, the full range of energy releases are available to him to conduct a full-scale weapons developement program.

Now I will resume with the statement, if this answers the question that you directed to Dr. Leonard earlier.

Representative HOLIFIELD. Go ahead, sir.

Dr. JOHNSON. The basic program I will refer to is that suggested in the Berkner Panel report of July 1959. A part of this program can be accomplished with chemical explosions. However, since the object of this program is to learn about the signals generated by nuclear explosions, these are technically the proper sources to use for at least part of the program.

Moreover, explosions greater than a few hundred tons can be staged more safely, more quickly, and more cheaply with nuclear explosives than with chemical explosives.

In response to your question considering the volume of chemical explosives, Mr. Chairman, a 20-kiloton explosion with high explosives

would occupy a volume of 20,000 cubic yards. It is about a yard per ton.

Representative HOLIFIELD. In other words, HE explosives require about a cubic yard to a ton?

Dr. JOHNSON. Yes; that is a good explosive like TNT. Ammonium nitrate takes a somewhat larger volume.

Representative HOSMER. That would be about 180,000 cubic feet.

Dr. JOHNSON. It is 20,000 cubic yards. That would be 540,000 cubic feet, sir.

Representative HOSMER. That is equivalent to a pretty good sized office building.

Dr. JOHNSON. That is a pretty good size; 20,000 cubic yards would be 540,000 cubic feet.

Representative HOLIFIELD. Give it to me hundred by hundred, that type of description.

Dr. JOHNSON. This would be a cube about 27 yards on a side, I believe. It is about 27 yards on a side, or a little bit less. A cube with edges, 80 feet long. It is a good sized room.

Representative HOSMER. About one-fourth of a football field.

Dr. JOHNSON. Yes.

Representative HOSMER. About one-fourth of a football field long and one-fourth a football field wide and one-fourth a football field high.

Dr. JOHNSON. Yes.

Representative HOSMER. It would take a pretty big hole to put that in.

Dr. JOHNSON. Yes. The construction of such a hole is expensive and time consuming.

Representative HOLIFIELD. A 20-kiloton nuclear would be about what cube?

Dr. JOHNSON. We have indicated that we can produce 20 kilotons in a package 30 inches in diameter without specifying the length. It is a very much smaller package. The Blanca explosion, which was 20 kilotons, for example, was fired in a tunnel which had a cross section of 9 square feet and the shot chamber itself was 20 feet long.

This is unclassified information that has been released. You get 20 kilotons in a room which is 3 yards on a side and 6 yards long.

Representative HOLIFIELD. That would be 9 by 18 feet.

Dr. JOHNSON. Yes. I had a 9-foot cross section and a 20-foot section in length; 9 by 9 by 20 feet was the size of the shot chamber in which the Blanca event was fired. The explosive yield was 19 kilotons.

Representative HOLIFIELD. That gives us, I think, a pretty good comparison.

Dr. JOHNSON. It makes a tremendous difference as far as staging is concerned.

Representative HOLIFIELD. It makes a tremendous difference both in time and cost to construct a chamber of that size. You could construct a chamber of that size by lowering a high explosive charge in the bottom of a well, for instance. It would blow out a place 9 by 9 by 20 feet, I am sure.

Dr. JOHNSON. As I stated earlier, we can lower a 20-kiloton explosion down a drill hole 30 inches in diameter.

Representative HOLIFIELD. Yes. But the point I am making is that if you wanted to have a decoupling hole, a decoupling hole could be made at the bottom of a well with high explosives capable of taking care of this size nuclear test.

Dr. JOHNSON. Yes. But in this case the nuclear explosion would be close-tamped. It would not be a decoupled nuclear explosion. We would have to resort to the techniques described by Mr. Meade or by mining operations to produce a large enough cavity to decouple a nuclear explosion, if I understood your question.

Representative DURHAM. Mr. Chairman, I do not think anyone has testified that we would learn too much from an explosion of the ordinary chemical nature, as far as I heard. It is not satisfactory. Dr. Latter just told us that you could not learn what you want to know from a chemical explosion.

We are just wasting time and money if we went to that. We might learn a little something.

Dr. JOHNSON. Yes. As Dr. Latter pointed out, there were some things that could be learned with high explosives, but there are many important things that cannot, and they are a lot harder to do.

Representative DURHAM. The important thing is what we want.

Dr. JOHNSON. Yes, sir.

Representative DURHAM. If we are going to build up a system that will cost millions of dollars.

Representative HOLIFIELD. You were the director at the Hardtack series, were you not?

Dr. JOHNSON. Yes, sir.

Representative HOLIFIELD. And you are now the director of Livermore?

Dr. JOHNSON. I am associate director for weapons testing and also for the Plowshare program, sir.

Representative HOLIFIELD. You have given a great deal of thought to the explosive properties of nuclear charges for canals and harbors and all of the things that we have been thinking about.

Dr. JOHNSON. Yes, sir. In great detail.

Continuing with my statement, any test explosion program must provide signals to answer a number of questions now not answerable.

First and primarily, it must give data for equipment evaluation and signal interpretation to establish whether readable differences exist which will permit differentiation of an explosion from a natural event, and if so, within what yield limits.

In accomplishing this, however, it must provide answers to a series of subsidiary questions. How does the signal generated and transmitted from an explosion vary with geographical location, and the manner of placement? How does it vary with the type of rock in which fired? How does it scale with differing explosive yields?

If data from chemical explosions are to be used in evaluations, what are the differences of signal to be expected as between chemical and nuclear explosions? What is the practicality of decoupling nuclear explosions by use of large cavities?

Naturally, a test explosion program to provide precise answers to all of these questions could be tremendous in scope. It could involve firing of all possible yields, both nuclear and chemical, under many conditions of placement and in many locations.

Yet such a program would be completely unreasonable. Therefore, over a period of several months we in the AEC's laboratories have discussed what might be a reasonable minimum explosion program to fit with the planned instrument and system studies.

Lately, in meetings with AFTAC, seismologists from outside Government, and other experts, we have tried to lay out what would be a minimum program, but one which would give reasonable answers in a period of 18 months to 2 years. I think we generally agree that the necessary program would involve the following:

(*a*) First, a series of 10–20 high explosive detonations of the order of 100 tons in various locations to investigate how signals vary with differences of location and placement. These could begin in the very near future. Exactly how many there would be and over what time period would depend on what the early shots revealed.

(*b*) At one location there should be nuclear shots of near equal size in two types of rock which are radically different; and a comparative high explosive shot. The purpose of that is to get a crossover between the nuclear experience and the larger number of high explosive experiments.

These three large shots, two nuclear and one HE, could be accomplished most rapidly and economically at the Nevada test site. Here we have a hard, dry granite outcropping close to a most, relatively soft volcanic tuff. Existing tunnels would permit us to fire within 9 months a nuclear shot of several kilotons in the tuff, a similar nuclear shot in the granite, and a comparative high explosive shot in the same geologic and geographical location.

To allow a reasonable test of distant investigation of the seismic signal, these shots should be of the order of 5 kilotons. They could be fired this fall and early winter.

Representative HOLIFIELD. Do we have enough types of underground material to cover the scope of types of earth that we would need?

Dr. JOHNSON. You mean at the Nevada test site?

Representative HOLIFIELD. No. I mean within the continental United States or the free world.

Dr. JOHNSON. I believe so, sir. I am not a geologist, but I believe that is so. Another medium we would like to investigate would be salt. Of course, here the proposed gnone experiment of the Plowshare program will provide such a seismic source in that medium.

Another medium of great interest would be limestone so that a shot in massive limestone would be a useful experiment.

Representative Holifield. We have plenty of limestone.

Dr. Johnson. Yes, sir.

I am now down to item (*c*).

(*c*) To understand how the signals scale with yield, shots of greater and lesser size should be tried and probably in this same location. Two to three shots at the NTS could serve this purpose. These could be conducted within a period of 9 to 12 months by using existing tunnels. Probably one of much higher yield—20 kilotons or more—and one or two of lesser yield—near 1 kiloton and less—should be exploded.

(*d*) At least one explosion of approximately 5 kiloton equivalent, probably nuclear, should be fired in a seismic area to examine the

signal differences from explosions and earthquakes for occurrences in such an area. To select the site for such a firing and to emplace it probably would require 12 to 18 months.

(*e*) High explosive firings in a relatively small cavity indicate that decoupling will occur. Yet these were small, high explosive firings. A few nuclear firings must be tried in one or more cavities—with tamped firings for calibration in the same location—to see the nature and degree of decoupling achieved for signals from nuclear explosives.

This should be attempted both for shots small enough to be fully decoupled by the chamber, and for shots somewhat larger to examine partial decoupling.

The initial program outlined here could be accomplished in 18 months. It is improbable that it would provide precise answers to all questions, yet the results should make a very substantial contribution to the efforts of the United States to improve the proposed seismic detection system and understand its capabilities.

That completes my statement.

Representative HOLIFIELD. Thank you very much.

Dr. Johnson, could a nuclear test device be constructed without weapon characteristics for the purpose of a joint United States and U.S.S.R. and United Kingdom series of tests for the purpose of determining earth type characteristics and seismological registrations?

Dr. JOHNSON. You mean the design and construction of a special device that could be fully exposed, opened up?

Representative HOLIFIELD. You and I are both aware of the fact that the Soviet position is one of having no more nuclear tests of any kind for any purpose, with the possible exception that they may have agreed in principle to a Plowshare type of experiment? Am I right in that?

Dr. JOHNSON. Yes, sir.

Representative HOLIFIELD. The fear of any nation would be that the other nation was furthering its weapon development if they had additional nuclear tests of any kind. The point of my question is, Can nuclear tests be held in which the three nations would be willing to allow each other to see the device, to know the component materials in it and therefore assure each nation that there was no weapon knowledge advantage in the type of test.

This would undoubtedly rest upon an agreement between the nations as to what type of a device was used.

Dr. JOHNSON. Yes, sir; I understand the question.

From a general standpoint we would believe that the best way to proceed would be to transfer a stockpile of standard packages of some sort to international custody. Not necessarily open the boxes up, but provide a set of boxes containing a set of explosives of different yields, in a manner similar to what we have proposed for the Plowshare program.

Representative HOLIFIELD. But wouldn't this be susceptible to trickery?

In other words, let us just assume that we had a five kiloton design of a weapon that we wanted to test and put it in a black box and box it up and submit it for an international agency to use. We would record the phenomena that occurred on that shot. That would mean something to us in relation to a design where it would not mean so much to a nation that did not know what the design of the device was.

Dr. JOHNSON. Yes, that is true. There is this point that could be made, however. Using such a package, another way of monitoring the explosion to make much surer that it is not a weapons development experiment is the examination of the kind of instrumentation that is used.

For example, we know that to do a weapons development experiment we have to have certain kinds of very high time resolution instrumentation. Such instrumentation is not essential to the seismograph program. So the absence of such instrumentation would clearly indicate that you were not making those kinds of important measurements and that would certainly be convincing to technicians of the U.S.S.R. and the United Kingdom. There is one bit of information, however, that you would get out of the explosion which is needed for the seismic program. That is the energy release.

Representative HOLIFIELD. You would get the energy released and you would also get the knowledge that a design of that type did explode.

Dr. JOHNSON. Yes, sir.

Representative HOLIFIELD. That it was a successful explosion.

Dr. JOHNSON. Yes, sir.

Representative HOLIFIELD. Therefore, it would seem to me that if you wanted to make a device that was, let us say, 2 feet in diameter and 3 feet long that would yield 7 kilotons, let us say, just picking figures out of the air, and it was of a peculiar construction internally which if successful would give you knowledge to build a whole family of weapons, is it not true that this would be in effect a valuable addition to weapon technology?

Dr. JOHNSON. Yes, sir; I would have to agree it would be an addition. The value of it would depend on the specifics of the situation.

Representative HOLIFIELD. Yes. Now I will get back to my original question: Is there any way in which the U.S.S.R. and the United Kingdom and the United States could sit down and agree upon a Plowshare type of program—and this is what this would be, for peaceful purposes—for the purpose of developing our techniques to see that no one cheats, is there any way that you know of, knowing that all three nations owning weapons, that any of the three nations would be willing to reveal their past and obsolete techniques by allowing the other nation to see exactly how the critical elements were spaced and triggered and so forth?

Dr. JOHNSON. In that connection, I don't know how the Nation would feel about it. My opinion would be that we certainly would not be running any risk to reveal to the U.S.S.R. and the United Kingdom some obsolete designs. We could do that for the purposes of this program without revealing any sensitive advanced weapons information that they probably do not already have.

Representative HOLIFIELD. Do you think we would be willing to do that?

Dr. JOHNSON. I don't know whether we would be willing to do that or not.

Representative HOLIFIELD. Do you think they would be willing to reveal to you any of their obsolete designs?

Dr. JOHNSON. I have no idea.

Representative HOLIFIELD. You see when we get down to the workability of these programs, we always come to the point of the willing-

ness of each side to give, in order to obtain the knowledge that we hope we could get. This is a technical question that I am asking. Can we join together in any kind of a nuclear explosion for Plowshare projects or anything else in which all sides would be assured that this did not contribute to advanced weapon technology?

Dr. JOHNSON. I doubt if you could do it in an absolute sense without giving a little bit. I think you would give certain kinds of information away if it was on us to provide the explosive. If the British were to provide the explosive or the Russians, you could ask any one of the three to provide explosives for this purpose and then work on that basis. I am talking about the seismic program alone. I think the seismic program is a little different from the Plowshare program technically.

Representative HOLIFIELD. It is different.

Dr. JOHNSON. But not politically in the sense that the explosions are specifically not for a weapons development program.

Representative HOLIFIELD. But you are going to run into the same problem on your Plowshare programs, too. You are going to run into a reluctance on the part of three nations to allow a Plowshare program for fear that the Plowshare explosions would give valuable information in weapon technology.

That is why I bring the question up.

Representative HOLIFIELD. You put a great deal of importance upon continued nuclear explosions. It is important to know if there is any possibility of the nations agreeing upon any kind of a technical device in which they would all three willingly say, "Here is a device. All of us know what it is and what is in the box. We know that its planned strength is 5 kilotons and when it explodes it may be 6 or it might be 4." But at least they would know in advance it was planned for 5 and they would know it produced either 4 or 6. They would know what produced that particular phenomenon.

Dr. JOHNSON. I think that such a system is certainly possible. It might be desirable to build a special explosive system. I think it is possible to do that on a short time scale and in a design which all three powers might agree was not very revealing with respect to advanced weapons technology.

Representative HOLIFIELD. It is a matter of bringing together a critical mass?

Dr. JOHNSON. Yes, sir.

Representative HOLIFIELD. But it goes far beyond that. It goes into what are the component factors. What is the mixture of each part of the critical mass? I assume you would have no trouble with the mechanical device of bringing them together. That could be very simple.

Dr. JOHNSON. Yes, sir. These things could be done.

Representative HOLIFIELD. Has there been any exploration with them in your Plowshare discussions? Have there been any explorations of the Russian attitude on this?

Dr. JOHNSON. First of all, I have not been involved in that part of the discussions firsthand myself. So all the information I have would be hearsay. But I do know that in the discussions we have talked about two concepts. One is to transfer a stockpile of boxes of various yields. The other is to use completely openable devices. The alternatives have not been discussed in detail with the Russians

as far as I know, or has anything been agreed to in principle with respect to which course would be taken either by the Russians, the United Kingdom or ourselves.

Representative HOLIFIELD. Are you speaking about Plowshare?

Dr. JOHNSON. Yes, sir.

Representative HOLIFIELD. So there has been no agreement in principle with any of the Soviet people as to even the type of procedure under the Plowshare program.

Dr. JOHNSON. No, sir. The Russians prefer to have us provide systems which we could open up completely.

Representative HOLIFIELD. They would prefer it.

Dr. JOHNSON. Yes. That is as I understand it.

Representative HOLIFIELD. What is our attitude? This is getting to the point I am talking about.

Dr. JOHNSON. Our attitude is this: That if we are to start soon on Plowshare in the sense of good economics, then we would prefer to use our more advanced systems. These are just the systems that we would not want to reveal.

Representative HOLIFIELD. These are just the systems that would give us more information in weapon technology.

Dr. JOHNSON. This assertion could certainly be made. So this is the quandary.

Representative HOLIFIELD. This is the quandary. Why, may I ask you, are you not willing if you do want Plowshare experiments, to reach an agreement with them whereby you would say each one of us will prepare a device which will produce 5 kilotons of power and each one will bring to the testing grounds our separate devices? Each one will reveal to the other his separate devices. Therefore, there will be a frank exchange of knowledge and either side could make it just as obsolete as they want to.

Dr. JOHNSON. I think that kind of approach would work for the seismic program.

Representative HOLIFIELD. I know it would work but would they agree to it?

Dr. JOHNSON. I could not answer that. I don't know.

Representative HOLIFIELD. Are there any further questions?

If not, we appreciate very much, sir, your testimony, as usual, and if you wish to supplement it in any way you have the opportunity.

Dr. JOHNSON. Thank you, sir.

Representative HOSMER. Mr. Chairman, I am going to have to leave.

Representative HOLIFIELD. Mr. Beyer, could I have your attention? I asked you to submit certain material and you may either respond to this at this time or add it to the material I asked you to submit. We recognize that there is no line item for Project Vela in the fiscal 1961 budget. What would you need in the way of funds in 1961 to prosecute the program at a high rate of progress?

Mr. BEYER. I would like to comment on that if I could.

Representative HOLIFIELD. Could you comment on it at this time?

Mr. BEYER. Yes, sir.

I think I would like to say in preliminary fashion that these values which I will give you are estimates of what we think we could use profitably, but which are not yet checked out through all of the necessary review points within the departments. This is going on at the present time.

Representative HOLIFIELD. This is, you might say, a kind of summary of your present working plan?

Mr. BEYER. It is a working estimate, yes. The DOD costs for the fiscal 1961 Vela uniform program would total roughly $27 million.

Representative HOLIFIELD. That is for fiscal 1961?

Mr. BEYER. Yes, sir. That would include the seismic research and development and the DOD part of the technical program cost in connection with the program of nuclear and chemical detonations. The surface based program would cost approximately $3 million. The satellite research and development program known as the Vela Hotel program would, if authorized, cost approximately $10 million. So in round numbers the total would be about $40 million for DOD costs for fiscal year 1961.

We have looked into this with some care and we believe we could use this money wisely and get results with it.

Representative HOLIFIELD. Would it be premature for me to ask you to submit, if your superiors have no objection, an itemized list of how you break down this $40 million?

Mr. BEYER. I will ask them.

Representative HOLIFIELD. It doesn't necessarily have to be for the public record. I would like to have this for committee information.

Mr. BEYER. I will ask them.

Representative HOLIFIELD. As it is in temporary form.

Mr. BEYER. Yes, sir. Then there is the other part of the fiscal year 1961 program which involves AEC costs but I cannot speak about them in any great detail. However, the combined package of DOD and AEC costs for fiscal year 1961 for the Vela Uniform, Sierra, and Hotel would be on the order of $80 million.

Representative HOLIFIELD. Thank you very much.

Representative HOSMER. Mr. Chairman, 2 days ago, I think it was, unanimous consent was requested to insert an article on digging in salt by some professor, the item having been printed in the Bulletin of the St. Louis Committee for Nuclear Information, or some such name. I asked that it be laid over at that time. I wanted to talk to Mr. Price about it because I feel that the record should be substantiated to the extent that it can be by the evidence being presented by the persons whom we know and can cross-examine. I have been unable to talk to Mr. Price and as a courtesy to him I will not object at this time to that going in the record with the understanding that it constitutes neither a substantiation of the information, approval of the committee, or anything else about it. (See attachment 4, p. 426.)

Representative HOLIFIELD. I think that is very generous of you, Mr. Hosmer. I am sure Mr. Price will appreciate it.

Representative HOSMER. Mr. Chairman, there is one other thing I think these records ought to show. They will be printed and they will be distributed to anybody who wants to see them. During the entire period of the hearings we have been in public rooms, the doors have been open, there have been hundreds of people in and out, who have heard the hearings. Nobody has been kept out during the period of the hearings.

I see here in the room a representative of the Soviet Embassy. That has been the case throughout the hearings. I just want to make it clear to anybody who reads these hearings that we operate in accordance with the tradition of open information and no secrecy here in the United States.

Representative HOLIFIELD. I think that is a very pertinent observation. We have been very happy to have our guests, including any members from the different embassies here. The complete and unexpurgated testimony will be printed. There will be additional items that will be submitted. In fact, there have been additional and detailed papers submitted by the different scientists, sustaining their points of view. They will be printed in the hearings.

Thank you, Mr. Beyer. We have one other gentleman who has requested to make a technical presentation on unmanned stations. Dr. Jehle, Physics Department, George Washington University.

Dr. Jehle, we will extend to you the courtesy of the Chair, although you were not on the original list of witnesses. I understand your presentation will be technical in nature.

STATEMENT OF DR. HERBERT JEHLE,[1] PHYSICS DEPARTMENT, GEORGE WASHINGTON UNIVERSITY

Dr. JEHLE. Mr. Chairman, members of the committee, may I go on from the proposal which was made by Dr. Bethé yesterday and state an objective—a technical objective. Mr. Wadsworth and other members of the delegation have encountered difficulty of negotiation in regard to any extension of the network—say to 600 stations. The main difficulty concerned the flooding of all countries with servicing inspectors.

Representative HOLIFIELD. Let me understand you, sir. Has the number 600 been a subject of negotiation by the Geneva negotiating representatives?

Dr. JEHLE. It could only have come up at the foothills conference.

Representative HOLIFIELD. At what conference?

Dr. JEHLE. At the foothills conference in London.

Representative HOLIFIELD. Just a minute, sir. What conference is that? I have not heard of this.

Dr. JEHLE. The foothills conference which convened at London, February 2 to 4, at which Dr. Orear, Stewart Meacham, George Kirstein, and Congressman Charles O. Porter attended.

Representative HOLIFIELD. This was not an official conference?

Dr. JEHLE. That was an unofficial conference.

Representative HOLIFIELD. This is an unofficial conference between who?

Dr. JEHLE. Between parliamentary-level politicians and individuals from 14 nations meeting unofficially in individual capacity. The conference was extensively reported in the press and in the Bulletin of Atomic Scientists. All my information comes either from the press or from Dr. Orear.

Representative HOLIFIELD. So there is a distinct understanding that this was not any official conference you are going to make a comment on, it is on the technical matter they discussed?

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Born, Mar. 5, 1907, Stuttgart, West Germany.
Education: Dr. Ing., Institute of Technology, Berlin, 1933.
European experience: Postdoctoral student, Cambridge, England, 1933–34; editorial collaborator, Jahrbuch u.d. Fortschritte d. Mathematik, Berlin, Germany, 1935–36 research assistant, University of Southampton, England, 1937–38; research associate, University of Brussels, Belgium, 1938–40;
American experience: Harvard University, instructor, 1942–46; designed projection magnifier for the semiblind under the auspices of the Committee for Sensory Devices, National Research Council, with Franklin Institute, 1946; Institute for Advanced Study, Princeton, member, 1947; University of Pennsylvania, 1947–49; University of Nebraska, 1949–59; California Institute of Technology, 1956–57; George Washington University, professor, 1959–.
Membership: Fellow, American Physical Society.

Dr. JEHLE. That is right. The technical matter is that the negotiability of several hundred unmanned stations essentially is tied to the possibility of making them serviceable by the manned control stations. The point is that from 21 manned control stations, the complete servicing and operation of hundreds of unmanned stations might be possible. I have checked up with experts on such a scheme and the consensus is that this is an objective which is in the realm of possible technical achievement.

Representative HOLIFIELD. In other words, that each 1 of the 21 stations could take care of something like 30 unmanned stations as far as checking them is concerned, maintenance, guarantee against, or at least apprehension of tampering?

Dr. JEHLE. Yes. Between the simple unmanned stations and the manned control stations there would have to be radio communications—to which the Russians objected. The objection was that radio communications would provide a guidance device for U.S. missile systems. But that is not a well-founded objection because the regular radio stations are better guidance devices. In case of an alert, the radio stations would be switched off and these control station radio networks would be switched off, too. There is no great technical difficulty in making foolproof keyed communications by radio between our control stations and the unmanned stations. The consensus of the experts again is that deliberate falsification of the unmanned stations' seismic records is practically impossible, provided an occasional inspection under the quota system would be possible. The main problem is how to read the great mass of seismic data which come in.

I would like to shortcut the testimony. I do not want to create the impression that tamperproof unmanned stations are a technically solved problem. Nor do I think that the issue of any additional seismic stations should be brought up at the present time, as we are to monitor only large explosions now. It is, however, considered by the experts as a problem which is far simpler than any of the communications problems we have in Explorers, or similar type of communications systems.

Representative HOLIFIELD. Again you are speaking of the experts that have gathered at the foothills conference?

Dr. JEHLE. Not only experts at the foothills conference but all the seismologists I was able to get in touch with in Washington and many outside Washington, D.C. Evidently it will take several years until we will have developed instrumentation and until the personnel will have been trained for monitoring small tests. The question has come up as to what type of experimentation should be used in a cooperative international study of detection techniques. The question was raised as to the possibility of conducting underground tests with nuclear explosives in order to learn more about detection techniques.

I would like to draw attention to a nontechnical item and that is that the outside countries, e.g. China and Egypt, will look very critically if the nuclear club goes on experimenting with nuclear weapons.

Representative HOLIFIELD. We are not going to get into that, Dr. Jehle. That is nontechnical, as you know. You have attended these hearings?

Dr. JEHLE. That is right.

Representative HOLIFIELD. I have tried very diligently to keep our discussions to technical points.

Dr. JEHLE. Yes.

Representative Holifield. I am extending you the courtesy of the Chair in order that you may present a technical matter.

Dr. JEHLE. That is right. I think the complete statement of the technical points is contained in my letter to Ambassador Wadsworth. There are no further comments to be made.

Representative HOLIFIELD. I have not read the letter. Are there other items in there of a nontechnical nature?

Dr. JEHLE. They are deleted from the record. I wrote that letter on March 10. I have since then discussed it with colleagues.

Representative HOLIFIELD. Are there other articles in your March 10 letter of a nontechnical nature, expressing conclusions or political or philosophical comments?

Dr. JEHLE. No; this is purely technical.

Representative HOLIFIELD. On your assurance we will accept it. If on perusal it goes beyond the technical area, in fairness to the other witnesses who have been admonished not to indulge in the philosophical or political field, we will have to eliminate it.

Dr. JEHLE. That is right. Thank you very much.

Representative HOLIFIELD. Thank you, sir.

(Excerpts from the letter submitted by Dr. Jehle follows:)

GEORGE WASHINGTON UNIVERSITY,
PHYSICS DEPARTMENT,
Washington, D.C., March 10, 1960.

Ambassador JAMES J. WADSWORTH,
Delegation of the United States, care of the American Consulate Nuclear Conference, Geneva, Switzerland.

DEAR MR. WADSWORTH: Thank you very much for your kind letter of February 25 in reply to my letter of February 22 in which I referred to the development of unmanned seismographic control stations.

"Without arguing the desirability of such control stations in any way," as you stated in your letter, you informed me that "the Soviet representative in Geneva has flatly turned down the idea (of unmanned seismographic control stations) on two separate occasions." And you mentioned that "the reason he gives is that this would provide the opportunity for many potential espionage agents to travel the length and breadth of the Soviet Union in servicing and checking on these stations."

* * * * * * *

As you know, the "foothills conference" in London, February 4, 1960, brought about a positive reaction from the Russians to the Western request for a closer study of the subject of unmanned seismic stations.

At the time of the American Physical Society meeting and the Federation of American Scientists meeting, held in New York during the last days of January, there were a good many discussions of the issues concerning these stations. It was clear from the onset that a clear-cut identification—not just some registration—of a small seismic signal as a very small nuclear explosion, is a difficult task if nothing but the 180 control stations' recordings were available. For this reason a great deal of discussion centered around the issue of unmanned seismic control stations.

In the meanwhile many things have happened at Geneva. Accordingly the arguments presented in this letter are altogether based on the assumption that a limited annual quota of inspector excursions would be permitted, to follow up suspicious seismographic or other recordings, and the arguments presented here are obviously restricted to the problem of monitoring of territories which are covered by the eventual test ban agreement.

The essential point is that we have to develop unmanned seismographic stations which can be left to be installed and routinely serviced by the nations of the country in which they are placed. Their proper placement and functioning would of course have to be checked:

(1) The check of the purely geographic location is easily obtainable as well by standard techniques of electrical communication engineering as by the responses of those unmanned stations to natural earthquakes.

(2) A check is needed in regard to the proper upkeep of the equipment, so that concealment cannot occur through simply letting the equipment rot. Together with that a check can be made on proper identification of the responses from unmanned stations. Such check can be achieved through the simple requirement of periodic replacement of the instruments to the manned stations which act as servicing agents and clearinghouses. The unmanned seismographs are sealed units to be opened at these regular inspections only, by the teams at the manned stations. The unmanned seismographs have their power supply from a battery enclosed within the sealed unit. They have radio communication with the manned control stations, and this communication can be keyed so that the manned control stations will get only the response of legitimate manned stations.

(3) A check is necessary against attempts to conceal adequate seismographic response to nuclear explosions by means of damping down, in one way or another, the unmanned seismographs; this may be a permanent damping, or it may be done solely at the moment a nuclear explosion is clandestinely performed. A check upon the normal orderly performance can be achieved by a comparison of the unmanned seismographs' responses to natural earthquakes with the simultaneously registered corresponding responses of the manned control stations, under surveillance of international inspectors. In earthquake regions there are enough of such quakes to provide for frequent checks. In earthquake-free regions one may rely on the responses at the times of major earthquakes as it has already been the consensus that in the latter regions tremors which are not clearly due to a distant earthquake would be subject to inspections.

As regards a momentary damping attempt to cheat, a simultaneous damping of unmanned stations at the occurrence of a secret explosion cannot but lead to suspicious recordings, particularly if there are several seismographs at each unmanned station, under regular exchange with the manned stations. An eventual inspection trip to the unmanned stations upon suspicious recordings cannot fail to reveal the rather elaborate setup necessary to achieve a temporary damping of seismographic recordings. While the inspection team sets out to check on unmanned stations, a removal of unmanned seismographs, say from a fake mounting equipped for occasional damping, to an honest site, would clearly be impossible because a removal of the instrument would show up in the recordings.

(4) To simply register a seismic signal, and to identify such a signal as due to an earthquake or as due to a nuclear explosion, are, in terms of kilotonnage TNT equivalent, orders of magnitude different items. The unmanned seismographs' fitness to identify tremors depends much on proper registration of the first motion. The sealed seismographs should, of course, not permit a quick reversal of the polarity of the response. As these seismographs are under periodic inspection through delivery to the internationally manned control stations, checks in this respect are possible. The difficult job of identification of a signal as coming from an explosion versus an earthquake can receive immense help through the use of a great number of unmanned stations. * * * The responses of the multitude of seismographs, manned and unmanned, yield a great deal of information, which is interlocked in a consistent fashion. An attempt at falsification of recordings would require the production of a highly coordinated, consistent set of false recordings, quite an impossible job. So, any attempt at cheating would only be possible by attempts at triggering a very small nuclear explosion at the very moment of a local earthquake. The problem of detection is accordingly one of establishing a large enough number of unmanned seismographs to frustrate the possibility of cheating without raising suspicions.

The manned stations are to be equipped with deep-hole seismographs (several thousand feet). Depending on the frequency of the vibrations, depending on the soil condition and depending on the kind of surface noise condition, this technique gives a great improvement over surface instrumentation.

There is a further point which I should like to bring up. The quoted New York Times report of February 19 refers to a phased ban proposal: "As the size of underground explosions that would be covered by the ban become smaller, more inspections would be required." I interpret this to mean that the U.S. delegation suggested that, with the lowering of the size of monitored underground explosions, a drastic increase of the number of actual inspection trips would be contemplated.

This formula contains a very dangerous pitfall. If we want to insist on monitoring all nuclear explosions down to presently unknown small sizes, we end up with a full-sized army of inspectors, an institution which, because of its colossal size, would be highly undesirable. It would lead to endless friction and troubles. A handful of brilliant, courteous, intelligent international inspectors is all that is needed; a nation which cheats on a nuclear test ban or moratorium will gain very little because continued small-scale testing will eventually be discovered. * * *

With my very best wishes for successful conclusion of the negotiations, I am

Sincerely yours,

HERBERT JEHLE, *Professor of Physics.*

Representative HOLIFIELD. In conclusion, today we concluded 4 full days of public hearings on the technical problems of a nuclear weapons test ban and the necessary detection techniques and inspection controls.

It was our stated intention through these hearings to bring to the Congress and the public in an understandable manner all available information on this subject. We attempted to avoid—as we said we would—general policy questions in the disarmament field, as well as philosophical discussions of nuclear weapons testing. This is not to say that these matters also are not of extreme importance. We must, if we are to understand and to arrive at intelligent and logical decisions, distinguish between what are technical problems and what are political problems.

During our 4 days of public hearings, we received oral testimony from over 20 scientific and technical experts—a list of the witnesses in the order of their appearance is attached (see attch. 2, p. 424)—all witnesses have been invited to supplement their oral testimony with additional statements pertinent to the overall testimony.

The subcommittee sampled a wide cross section of scientific opinion. Many of the witnesses were official advisers and consultants in the Geneva negotiations from the Experts Conference in 1958 through the two Technical Working Groups, (1) High Altitude and Space Detection and Identification, and (2) Improvement of Underground Detection and Identification. Many of the witnesses have been outspoken on both sides of the nuclear test ban controversy, but have confined themselves to the technical aspects.

An objective summary and analysis of the testimony will be prepared after thorough review of the record. Having listened to all the oral testimony and the panel discussion, which were part of the hearing, I believe I can make a few general comments.

First, I regret that a hearing of this type was not held more than a year ago. The information which has been assembled in these hearings would have been of inestimable value to Congress, and the public, as well as the executive branch, in understanding the complicated technical problems involved in the detection and control of nuclear weapon tests.

Second, it appears from the testimony that for the next several years at least it will not be possible to detect and identify underground bomb tests whose seismic signals record the equivalent of a nonmuffled bomb explosion of 20 kilotons, or less. Twenty kilotons is equivalent to 20,000 tons of TNT and is approximately the yield of the atomic bomb which destroyed Hiroshima in World War II. Further, it appears that for this same time period it will not be possible to detect muffled tests of 100 kilotons or more set off deep underground in large cavities. From that time forward, it will be a race between

improved means of detection and identificationas against improved means of concealing and muffling nuclear tests.

Third, it was the unanimous opinion of all witnesses that a vigorous and sustained program of research and development was necessary to improve our mechanical devices and our techniques of detection, identification, and inspection of nuclear explosion tests.

The committee stands adjourned.

(Whereupon, at 4:15 p.m., Friday, April 22, 1960, the committee was recessed subject to call.)

ATTACHMENTS

(The following material was referred to during the course of the hearings.)

ATTACHMENT 1

GLOSSARY

Accelerometer: An instrument which is so designed that it responds to the accelerations of the earth relative to the suspended mass of the instrument rather than measuring either the velocity or displacement of the earth. This instrument is usually employed to measure the close-in strong motion from an underground nuclear explosion.

Array: A group of seismometers arranged in a systematic pattern over an area of a few square miles for the purpose of increasing the detectability for small earthquake or explosion signals in the presence of background noise.

Backscatter radar: Detection of change in the characteristics of the ionosphere resulting from absorption by it or energy from a nuclear detonation. The change in ionosphere characteristics changes the intensity of the radio frequency waves emitted by the radar transmitter and reflected (scattered) back to the radar receiver.

Clear rarefactions: The first motion recorded by a seismograph as a result of a distant earthquake may be either compression or rarefaction. In the first case, the first motion is caused by a movement of the earth outward from the source and in the second case, by a motion of the earth toward the source. Clear rarefactions are those which are large enough in amplitude above background microseismic noise to permit these rarefactions being identified without question as movements of the earth toward the source of the earthquake.

Cosmic noise: Incoherent electromagnetic noise signals (static) originating in cosmic space and propagated inward through the ionosphere to the surface of the earth where they are recorded on sensitive radio receivers called riometers.

Coupling: In this report, coupling refers to the amount of the total energy released in an underground explosion which is transformed into seismic waves in the earth. The greater the coupling of a particular explosion the larger will be the seismic waves and the easier it will be to detect by seismographs at some distance from the explosion.

Cowboy Series: A series of chemical explosions in a salt dome in Louisiana which gave experimental proof of the theory of decoupling.

Decoupling: The process of reducing the size of the seismic signals from an underground explosion by detonation of the explosive in a large hole underground.

Deep focus: A term used by seismologists to indicate that an earthquake originates at some depth greater than 30 miles below the surface of the earth.

DEW line: **D**istant **E**arly **W**arning line. System of radar stations established in the most northerly areas of the North American Continent to provide early warning of possible impending attack by aircraft.

Direct optical: Detection of a nuclear explosion by use of instruments (photo multipliers) sensitive to short pulse of visible light emitted by the explosion as a result of incandescence of the matter in the device.

Electromagnetic: (Detection of) radio frequency waves caused by a nuclear explosion and propagated from the point of the explosion to great distances.

Electromagnetic induction detectors: An instrument which radiates a low-frequency electromagnetic field and is sensitive to any distortion in this field due to the presence of conducting material such as iron or copper pipe, wire, landmines, etc.

First motion criteria: Criteria (or tests) by which the nature of the source of a seismic disturbance is established. In principle, explosions will cause the first movement of the earth to be in a direction away from the source. Earthquakes, on the other hand, can cause the first movement of the earth to be in a direction toward the source. Specific criteria are phrased in terms of how much larger than the noise the apparent motion toward the source can be considered to be a reliable indication of an earthquake.

Fluorescence: Detection of a nuclear explosion by use of instruments sensitive to the short pulse of visible light emitted by the gases of the upper atmosphere in fluorescence caused primarily by absorption of energy from X-rays radiated from a nuclear explosion at high altitude or in space.

Gamma ray: Electromagnetic radiation similar to X-rays but generally more penetrating.

Hardtack II: A series of underground nuclear explosions conducted in the Nevada test area in the fall of 1958. The principal underground explosions were Blanca (19 kilotons) and Logan (5 kilotons). The seismic data from these explosions provide the basis of most estimates on the detection capabilities of the control system.

Heat-absorbing material: Materials such as powdered graphite may be distributed throughout the cavity in which a nuclear device has been mounted for the purpose of providing decoupling. Part of the energy of the explosion will be absorbed in vaporizing the powdered graphite and would theoretically reduce the pressure from the explosion on the walls of the cavity.

High frequency seismograph: A seismograph sensitive to seismic waves of from several cycles per second to about 100 cycles per second (principally used in prospecting for oil).

Hydroacoustic: (Detection of) pressure waves caused by an underwater nuclear explosion which are propagated through the ocean waters from the point of the explosion to an underwater microphone usually called a hydrophone.

Intermediate period seismograph: This term usually covers seismographs sensitive to seismic waves of periods between 2 and 10 seconds.

Ionospheric radar: See "Backscatter radar."

Kilometer: 1,000 meters (0.621 statute mile).

Kiloton: Explosive energy release corresponding to 1,000 tons of the chemical high explosive, TNT. The bomb detonated at Hiroshima

near the conclusion of World War II was equivalent in size to 20 kilotons of TNT.

Long period seismograph: A seismograph sensitive to seismic waves of natural periods greater than 10 seconds.

Magnetometer: An instrument for measuring the natural magnetic field of the earth to discover any distortion in this field by manmade ferrous articles (i.e., equipment, pipes, etc.) or the permanent magnetism which may exist in manmade instruments or natural ferromagnetic materials.

Magnitude: An arbitrary scale used by seismologists according to size. The scale is so arranged that a magnitude 6 earthquake is 10 times larger than one of magnitude 5; a magnitude 5 is 10 times larger than magnitude 4, etc.

Megaton: Equivalent of 1,000 kilotons or 1 million tons of chemical explosive (TNT).

Microseisms: Very small vibrations in the earth's surface layers caused by ocean waves beating on the shore and by the action of turbulent wind on the earth.

Nuclear debris sampling: Detection of nuclear explosions by collection of fine debris particles from the atmosphere by airborne or ground-air filtering equipment and analysis by radiochemical techniques.

Neutron: One of the elementary particles found in the nucleus of all atoms except the most abundant form of hydrogen. In the chain reaction fission process of a nuclear explosion, two to three neutrons are emitted for each uranium atom fissioned and are available to induce fission of more uranium atoms. These neutrons have high initial energies and are transmitted to great distances where they can be detected with suitable equipment.

Nevada tuff: A weakly cemented, rather crumbly rock, formed from volcanic ash. The underground nuclear explosions of Hardtack II, as well as the Rainier explosion, occurred in this rock, which is somewhat elastic and produces stronger seismic signals from nuclear explosions than are produced by the same size explosions in salt and granite.

Overburden: When a hole is excavated deep underground for the pourpose of decoupling a nuclear explosion, the earth or rock between the hole and the surface produces an external pressure on the walls of the hole in proportion to the depth. The amount of earth above the hole, usually measured in feet, is referred to as the "overburden."

Peak signal: The maximum amplitude of motion of the seismograph in response to a nuclear explosion or earthquake.

Piggyback: Experimental equipment for making measurements in space which is added to a missile or satellite originally planned for other types of measurements.

Rainier coupling: The Rainier shot (1.7 kilotons) conducted 900 feet underground in intimate contact with a rocklike material known as Nevada tuff on September 19, 1957, was estimated to have transmitted about 2 percent of its energy into seismic waves. This observed degree of energy transformation from the explosion to seismic waves in the earth (coupling) has been taken as a standard for subsequent calculations of seismic signals generated by underground nuclear explosions.

Reflection shooting: The process of studying the rock strata deep under the surface (20,000 feet) by recording on an array of surface seismographs the seismic waves from small chemical explosions which travel downward and are reflected back to the surface on near vertical paths.

Refraction shooting: The process of studying the detailed character of the strata underlying the earth's surface by firing small charges of chemical explosives and studying the seismic waves from the explosion which proceed downward through strata in question, are refracted (bent) along the strata and refracted again back upward to an array of seismometers where they are detected and recorded.

Seismograph: An instrument for detecting and recording small vibrations in the earth's crust. The detector portion, called a seismometer, usually consists of a mass suspended from a frame either by springs or as a pendulum, in such a manner as to tend to remain in one position while the earth-mounted frame moves with the earth. The relative motion of the frame and mass is usually magnified and recorded as a seismic disturbance either natural or manmade.

Seismometer: The detector portion of a seismograph which may be located in a deep hole or on remote outcrops as in array emplacements and its motion being transmitted as electrical pulses through many miles of cable to the recording instrument.

Shielded detonation: Detonations of nuclear explosives in space can be "shielded" by placing thin spherical or cylindrical sheets of lead or other material around the nuclear device. This shield reduces the amount of radiation from the nuclear explosion and hence reduces the range of detection. Shields are particularly effective for thermal X-rays.

Short period seismograph: A seismograph sensitive to seismic waves of about 2 seconds or less (of considerable significance in detecting nuclear explosions).

Signal-to-noise: In recording the vibrations produced in the earth by a nuclear explosion it is necessary that the size of the signature from the explosion be larger than the microseismic noise on the record to permit the signature to be identified. The ratio of the amplitude of the signal to the amplitude of the noise is called the "signal-to-noise" level.

Strategic nuclear weapons: Those nuclear weapons ranging in yield of approximately 50 kilotons and more, to include the megaton yields. This class is often referred to as "high yield weapons."

Tactical nuclear weapons: Those nuclear weapons ranging in yields below 20 kilotons. This class is often referred to as "low yield weapons."

Tamped shot: An underground explosion detonated in close contact with the surrounding earth or rock so as to transmit to the earth or rock the largest possible fraction of its energy. (Alternative to maximize coupling.)

Technical Working Group I: Of the Conference on the Discontinuance of Nuclear Weapons Tests, the Technical Working Group on the Detection and Identification of High Altitude Nuclear Explosions (June–July 1959).

Technical Working Group II: Of the Conference on the Discontinuance of Nuclear Weapons Tests, the Technical Working Group on

the Detection and Identification of Underground Nuclear Explosions (November–December 1959).

Thermal X-ray: Electromagnetic radiation due to the extremely high temperature of the nuclear device fireball. Similar to normal X-rays but much less penetrating.

Threshold: A seismograph of a given sensitivity placed a known distance from an underground explosion or earthquake will record relatively large earthquakes and underground explosions. As the magnitude of the earthquake or equivalent yield of the explosion is reduced it becomes more and more difficult to detect the signature of these events on the seismograph. Eventually one reaches a "threshold" which is defined as the smallest earthquake or underground nuclear explosion which can be clearly detected.

Velocity meters: Strong motion instruments which are so designed as to record the velocity of earth motion relative to the suspended mass of the instruments rather than measure the actual displacement of the earth.

ATTACHMENT 2

PUBLIC HEARINGS ON NUCLEAR TEST DETECTION (APRIL 19–22, 1960)

LIST OF WITNESSES IN ORDER OF APPEARANCE

1. Dr. Harold Brown, Lawrence Radiation Laboratory, Livermore, Calif.
2. Dr. Wolfgang H. Panofsky, High Energy Physics Laboratory, Stanford University, Palo Alto, Calif.
3. Dr. Richard Latter, Rand Corp., Santa Monica, Calif.
4. Dr. Carl F. Romney, Headquarters, USAF, Air Force Technical Applications Center, Washington, D.C.
5. Dr. Albert R. Latter, Rand Corp., Santa Monica, Calif.
6. Mr. L. P. Meade, Phillips Petroleum Co., Bartlesville, Okla.
7. Dr. Edward Teller, Lawrence Radiation Laboratory, Livermore, Calif.
8. Dr. Hans Bethé, Physics Department, Cornell University, Ithaca, N.Y.
9. Dr. Jack E. Oliver, Lamont Geological Observatory, Columbia University, New York, N.Y.
10. Dr. Alvin C. Graves, Los Alamos Scientific Laboratory, Los Alamos, N. Mex.
11. Dr. Richard E. Roberts, Carnegie Institution, Washington, D.C.
12. Dr. Jay Orear, Physics Department, Cornell University, Ithaca, N.Y.
13. Dr. Roland Beers, seismic consultant, Troy, N.Y.
14. Dr. Dean S. Carder, Chief Seismologist, U.S. Coast and Geodetic Survey. Washington, D.C.
15. Dr. Harold Urey, University of California, Scripps Institute of Oceanography, La Jolla, Calif.
16. Dr. Charles E. Violet, Lawrence Radiation Laboratory, Livermore, Calif.
17. Dr. Richard M. Foose, Department of Earth Sciences, Stanford Research Institute, Menlo Park, Calif.
18. Dr. Raymond A. Peterson, United Geophysical Corp., Pasadena, Calif.
19. Mr. Frank B. Coker, United Electro Dynamics Corp., Pasadena, Calif.
20. Dr. Byron P. Leonard, Space Technology Laboratory, Los Angeles, Calif.
21. Dr. Richard Taschek, Los Alamos Scientific Laboratory, Los Alamos, N. Mex.
22. Mr. Carleton M. Beyer, Advanced Research Projects Agency, Department of Defense, Washington, D.C.
23. Dr. Gerald W. Johnson, Lawrence Radiation Laboratory, Livermore, Calif.
24. Dr. Herbert Jehle, George Washington University, Washington, D.C.

ATTACHMENT 3

ADDITIONAL MATERIAL SUBMITTED BY DR. CARL F. ROMNEY, AFTAC

During the meetings of Technical Working Group 2 at Geneva in November and December of 1959, more than 100 seismograms containing recordings of the Logan explosion were given by the United States to the United Kingdom and U.S.S.R. delegations. These seismograms were obtained from 54 stations in the United States and Canada. Similar sets of seismograms from other major explosions were also given to the other delegations. The purpose of supplying these records to the other delegations was to enable them to evaluate for themselves, using the same data which the United States had used, the U.S. conclusions on detectability of underground nuclear explosions.

It should be pointed out for the record that the statement of the Soviet delegation, "In the opinion of the U.S. seismologists themselves, seismograms from only 16 of these 28 stations could be used for analysis" is, in the most charitable view, a half-truth. The U.S. delegates had made it clear in the meetings that this was not the case; if the Soviet delegation had conducted even the most rudimentary evaluation of the seismograms given to them, they would have known that this was not the case. The facts are these:

(1) Each seismogram had been analyzed by the United States, and each made its contribution to the resulting evaluation of detection capability. In many cases, the contribution was simply to confirm the extreme difficulty in detecting small explosions. In many other cases, the analysis consisted of the study of the direction of first motion, the relative size and frequency of the various body and surface waves, and other studies which do not require precise instrumental calibration.

(2) At 28 stations, the instruments were calibrated with sufficient precision to supply invaluable information on signal amplitudes. All of these data were used and the results were presented at the meetings.

(3) At 16 stations, the data were additionally useful for magnitude determinations. These 16 stations used for computing magnitude are listed below:

Station name:	*Distance from the explosion* *Kilometers*
1. Tinemaha, Calif	181
2. Woody, Calif	289
3. Riverside, Calif	371
4. Pasadena, Calif	382
5. Mount Hamilton, Calif	483
6. Barrett, Calif	503
7. Palo Alto, Calif	530
8. Berkeley, Calif	540
9. San Francisco, Calif	557
10. Mineral, Calif	583
11. Tishamingo, Okla	1, 804
12. Antlers, Okla	1, 902
13 Hot Springs, Ark	2, 111
14. St. Genevieve, Mo	2, 305
15. Providence, Ky	2, 506
16. Bangor, Maine	4, 021

From Nuclear Information, vol. II, No. 6; Greater St. Louis Citizens' Committee for Nuclear Information]

ATTACHMENT 4

DIGGING THE BIG HOLE

(By John N. Ong, Jr.[1])

The latest idea for avoiding detection of a nuclear bomb test is to hide the explosion by setting it off in a large underground hole. The theory is that in such an explosion the force of the bomb will be muffled, and produce only a small earth shock which may escape detection by seismographs.

This new proposal, suggested by Dr. Edward Teller, was studied by a group of physicists of the Rand Corp. Their six-page report published in classified form on March 30, 1959, and declassified on October 20, 1959, describes the theory in mathematical terms, and calculates that one could reduce 300-fold the seismic shock from a nuclear explosion. For example, an explosion of 100,000 tons could be made to look seismically like one of approximately 300 tons. To do this would require that the explosion be set off in a hole 800 feet in diameter, located in hard rocklike material, about one-half mile below ground.

Although the Rand report proves, in theory, that the bomb explosion can be hidden in a hole, it does not consider the practical problem of digging such a big hole without being detected. The practical side of the matter is a problem in engineering. The following is an engineer's analysis of what it would take to dig a hole 800 feet in diameter.

1. General

The theory requires that the hole be dug in hard material. Blasting a hole in hard rock would be the slowest and most expensive method. Also, it would be hard to hide such a big mining operation, and to conceal the pile of excavated rock from observation. The most practical method is to wash out a hole by pumping water through a large underground salt deposit—a salt dome. We shall therefore consider the latter procedure.

2. Location

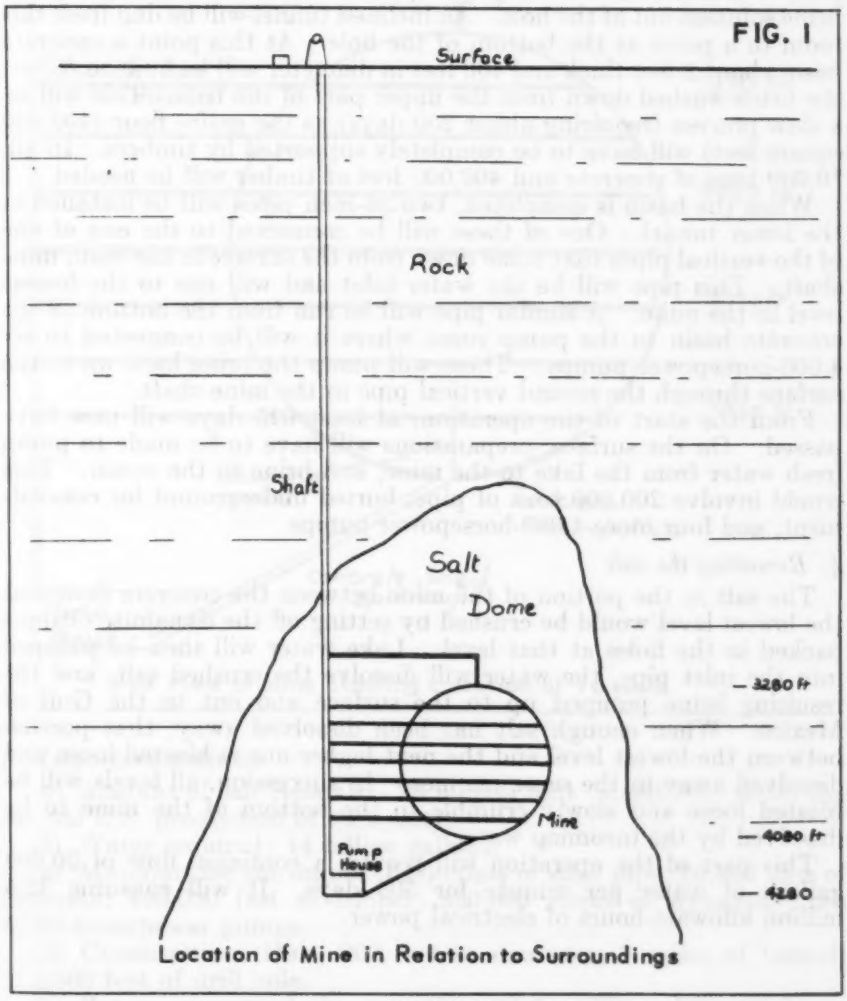
There are probably about a dozen suitable salt domes in the United States. (Russia appears to have none large enough.) The operation requires that about 14 billion gallons of fresh water be used to dissolve out the salt. Such a large amount of brine could only be disposed of in the sea. Accordingly, the operation must be located at a salt dome which is reasonably near a source of fresh water, and also near the sea. In the United States, one such location is in a region west of Lake Calcasieu, La., about 20 miles north of the Gulf of Mexico. We shall assume that the operation is located at this site.

3. Mining operations

The underground site of the hole will be reached from the surface by means of a mine shaft. The shaft will have to be dug first—nothing else can be done until that is finished. All the rock and salt

[1] Dr. John Ong is assistant professor of mechanical engineering at Washington University, St. Louis.

(30,000 tons) removed in digging this shaft will have to be loaded into buckets and hauled to the surface to be trucked away. At the same time, two 30-foot steel pipes to carry water (for the washing out operation) will have to be put in, and the sides of the shaft cemented. The best possible speed for sinking this shaft is about 10 feet per day. To bring the shaft to the level of the top of the hole will require about 328 days of work, day and night, 7 days a week.



At this time the necessary development of the mine can be started. The general plan is to dig horizontal tunnels through the salt dome and to build a concrete basin where the bottom of the hole is to be. (See fig. I and II) then, layer by layer, the salt is crushed by setting off explosives in each horizontal tunnel, and the crushed salt is washed down to the basin and pumped out of the hole.

Seven tunnels, 100 feet apart (fig. II) will be cut horizontal to the main shaft, and a network of passages started on each level. Workers

will then start drilling holes 50 feet long and 50 feet apart, in the sides, top, and bottom of each tunnel. These holes will be loaded with explosive. As the main shaft is continued down, each level will be opened up and developed in the same manner.

After about 420 days the mining operation will reach the eighth level, 4,230 feet below the surface, or the location of the bottom of the hole. Here a large underground room will be dug (50 by 100 by 20 feet) to accommodate six huge pumps that will be needed to pump the brine solution out of the hole. An inclined tunnel will be dug from this room to a point at the bottom of the hole. At this point a concrete basin about 2 feet thick and 400 feet in diameter will be built to collect the brine washed down from the upper part of the hole. This will be a slow process (requiring about 200 days) as the entire floor (400,000 square feet) will have to be completely supported by timbers. In all, 70,000 tons of concrete and 400,000 feet of timber will be needed.

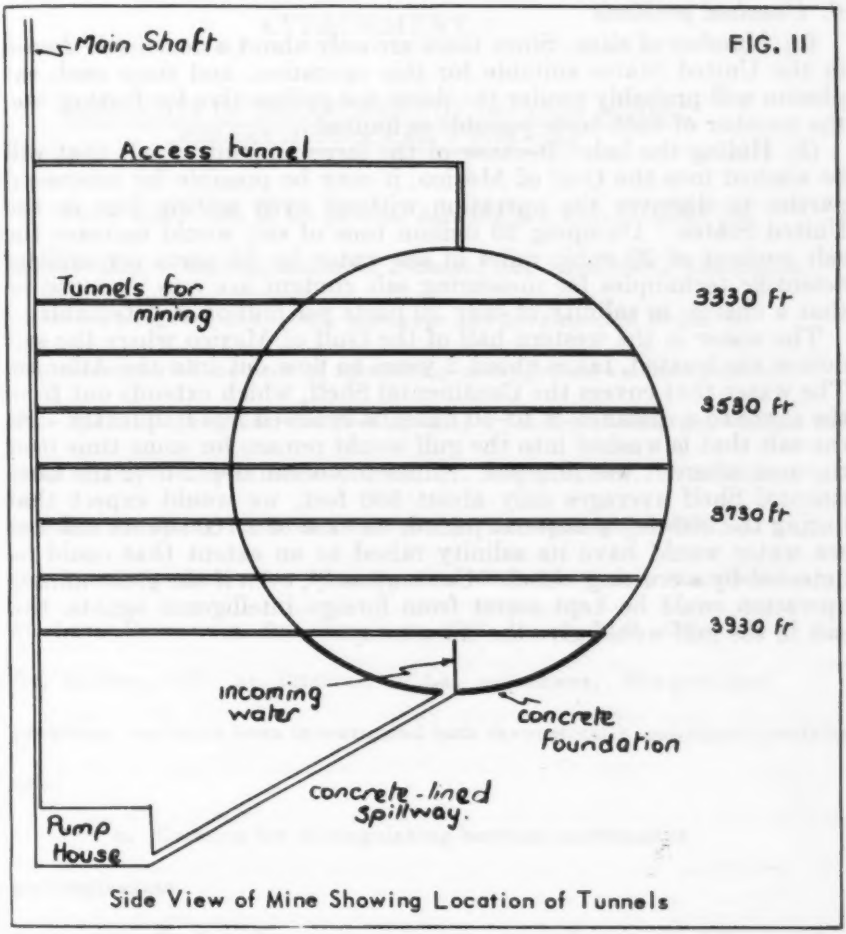
When the basin is completed, two 36-inch pipes will be installed in the lower tunnel. One of these will be connected to the end of one of the vertical pipes that come down from the surface in the main mine shaft. This pipe will be the water inlet and will rise to the lowest level in the mine. A similar pipe will be run from the bottom of the concrete basin to the pump room where it will be connected to six 4,000-horsepower pumps. These will pump the brine back up to the surface through the second vertical pipe in the mine shaft.

From the start of the operation, at least 625 days will now have passed. On the surface, preparations will have to be made to pump fresh water from the lake to the mine, and brine to the ocean. This would involve 200,000 tons of pipe, buried underground for concealment, and four more 4,000-horsepower pumps.

4. Removing the salt

The salt in the portion of the mine between the concrete floor and the lowest level would be crushed by setting off the dynamite charges packed in the holes at that level. Lake water will then be pumped into the inlet pipe, the water will dissolve the crushed salt, and the resulting brine pumped up to the surface and out to the Gulf of Mexico. When enough salt has been dissolved away, that portion between the lowest level and the next higher one is blasted loose and dissolved away in the same manner. In succession, all levels will be blasted loose and slowly crumble to the bottom of the mine to be dissolved by the incoming water.

This part of the operation will require a continual flow of 20,000 gallons of water per minute for 500 days. It will consume 32.5 million kilowatt-hours of electrical power.



5. Important statistics

(*a*) Amount of salt removed: 20 million tons (about four times the annual U.S. production of rock salt).

(*b*) Water required: 14 billion gallons.

(*c*) Materials for the mine: 16,000 tons of steel pipe; 70,000 tons of concrete; 400,000 feet of timber; 350,000 sticks of dynamite; ten 4,000-horsepower pumps.

(*d*) Construction: Hole, 800 feet in diameter; 7 miles of tunnel; 175,000 feet of drill hole.

(*e*) Power consumed in pumping: 32.5 million kilowatt-hours (enough to supply Greater St. Louis for 10 days).

(*f*) Number of workers: about 200.

(*g*) Estimated minimum time for completion: 3 years, 4 months.

(*h*) Estimated cost: about $50 million.

6. Unsolved problems

(*a*) Number of sites: Since there are only about a dozen salt domes in the United States suitable for this operation, and since each explosion will probably render the dome too radioactive for further use, the number of such tests possible is limited.

(*b*) Hiding the hole: Because of the large amount of salt that will be washed into the Gulf of Mexico, it may be possible for interested parties to discover the operation without even setting foot in the United States. Dumping 20 million tons of salt would increase the salt content of 20 cubic miles of sea water by 25 parts per million. Scientific techniques for measuring salt content are now so sensitive that a change in salinity of only 20 parts per million is detectable.

The water in the western half of the Gulf of Mexico where the salt domes are located, takes about 2 years to flow out into the Atlantic. The water that covers the Continental Shelf, which extends out from the shore to a distance of 40–50 miles, is renewed less frequently. So the salt that is washed into the gulf would remain for some time near the area where it was dumped. Since the ocean depth over the Continental Shelf averages only about 600 feet, we would expect that during the 500-day pump-out period, an area of 1,760 square miles of sea water would have its salinity raised to an extent that could be detected by a cruising vessel. Consequently, even if the great mining operation could be kept secret from foreign intelligence agents, the salt in the gulf would give the show away.

ATTACHMENT 5

HIGHLIGHTS OF DEPARTMENT OF DEFENSE SEISMIC RESEARCH PROGRAM - 1948-1960

From April 1948 to the present date the Air Force, under the sponsorship of the Department of Defense, has conducted a reasonably intensive program of seismic research and development aimed at improvements of methods of identifying underground seismic events. This research program was developed by a panel including Dr. Roland Beers (Chairman), Dr. John Adkins, ONR, Dr. Perry Byerly, U. of Calif., Dr. Morris Ewing, Columbia University, Dr. Norman Haskell, AFCRC, Dr. Frank Neumann, Dr. Dean Carder, USC&GS, Father Macelwane, St. Louis University, Dr. Gutenberg, CIT, Dr. Benioff, CIT, Dr. Richter, CIT, Dr. Slichter, UCLA, and others. The principal problems that have been investigated both theoretically and experimentally are:

a. Criteria for distinguishing between earthquakes and explosions.

b. Studies of microseismic noise in various parts of the world.

c. Studies of seismicity in the USSR.

d. Methods of improving seismic signal-to-noise ratio by means of large arrays of seismometers.

e. Development of improved seismographs.

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f. HE explosion programs for determining characteristics of seismic wave generation and propagation.

g. Utilization of US nuclear explosions at the Pacific Proving Ground and at the Nevada Proving Ground to study generation and propagation of seismic waves.

h. Development of precision timing devices, phototube amplifiers, continuous development film recorders, long line transmission of seismic data, and other auxiliary equipment.

i. Development of seismometers for use in drill holes deep underground to reduce background noise.

Instruments and data developed by the above programs have been the basis to the negotiations for a Test Ban Treaty being conducted in Geneva. A particularly significant item of this program has been probably the most extensive seismic experiment of all time which was conducted during Operation HARDTACK II. Seismograph stations were installed at a series of locations extending eastward from the Nevada Test Site across the U. S. to Maine. Observations were made at more than 30 locations on the large underground nuclear explosions, Logan (5 KT) and Blanca (19 KT). This data was basic to the recent work of Technical Working Group 2 at Geneva in connection with the negotiations for a nuclear test ban treaty with the USSR and the UK.

The program highlighted above has involved expenditures of funds at a fairly constant annual rate during the period from 1948 to 1960. It is estimated that somewhat more than $8,000,000 has been expended on nuclear explosion seismology at many universities, government laboratories, and private industrial laboratories during this period.

ATTACHMENT 6

PART I

PLANNED UNITED STATES RESEARCH & DEVELOPMENT PROGRAM FOR IMPROVING THE DETECTION OF UNDERGROUND NUCLEAR DETONATIONS

11 May 1960

Introduction:

This is a summary of the planned United States program of research and development for the purpose of improving the detection of underground nuclear detonations. The summary includes the program for Fiscal Year 1960 and the planned program for FY 1961. This is a first part of a continuing research program. Detailed plans for the larger range future have not been prepared, since the results obtained during the initial years (FY's 60 and 61) are expected to yield significant, but unpredictable, results which will influence long range planning.

The funds allocated to various categories of projects are indicated in order to reflect the relative level of emphasis in the planned program. In keeping with U.S. budgetary practice, fund information is given by fiscal years (FY). FY 60 covers the period 1 July 1959 to 1 July 1960; and FY 61 covers the period 1 July 1960 to 1 July 1961.

Summary of Seismic Research and Development Program*

UNDERGROUND DETECTION PROGRAM	FUNDS FY 1960	FY 1961**
A. Task 1 - Standard Seismological Network		(Only totals are available)
1. Selection of standard equipment: Meetings held 29 March at National Academy of Science and with the Academy's committee on 11, 12 April in San Francisco to select equipment; report expected 10 May 1960	$ 50,000	None
2. Procurement of equipment: Will await selection recommendations from NAS committee: Procurement and distribution of equipment to be initiated by U.S. Coast & Geodetic Survey. The estimated increment for FY '60 is 50 stations; 75 for FY '61.	700,000	

* This research and development program is known in the United States as Project VELA-UNIFORM.

**Only subtotals are shown since the tasks and funds assigned are likely to vary due to results obtained. Unless otherwise stated work under each item is planned.

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
3. Technical installation: Will include defraying basic expenses of installations, some aspects of operation but most of these funds will be for assisting in technical aspects of installation.		
4. Seismic data analysis center: U. S. Coast and Geodetic Survey now performs routine analysis of worldwide seismic data. Funds indicated here will be used to augment U. S. C&GS capability.		
Standard Network TOTAL	$750,000	$2,625,000
B. Task 2 - Research Programs: Objectives are to improve seismic detection techniques and to broaden the seismic research base.		
1. Generation and Propagation of seismic waves: Contracts to be let primarily with universities. It is expected that these studies will take some time, but will provide needed basic data for improved detection of seismic events.		

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
(TASK 2 Cont'd)		
a. Funds obligated	(See Below)	
(1) Studies of seismic waves generated from small (less than magnitude 5) earthquakes. Studies of focal depth, generating mechanism and occurrence of aftershocks.	$415,000	
(2) Studies of frequency characteristics and directional spectrum of microseisms and study of methods for distinguishing seismic signals in presence of noise.	297,000	
(3) Study of phase velocity of long period surface waves in selected mountainous regions. Investigations of transfer functions for typical crustal structures.	100,000	
(4) Study of frequency spectrum and energy attenuation of seismic waves from small earthquakes and explosions.	177,000	

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
(Task 2 Cont'd)		
(5) Studies of inverse transforms to be applied to long-period waves and development of high speed digital techniques. Model studies.	$309,000	
(6) Investigation of phase velocity in central U. S. and studies of source mechanism using seismic Waves other than "P".	240,000 (for 6 & 7)	
(7) Theoretical and experimental studies of the mechanism of generating horizontally polarized shear waves from small explosions.	(see note on (6) above)	
(8) Model studies of radiation patterns around simulated faulting and explosive sources.	10,000	
(9) Studies of the generating mechanism of seismic sources using intermediate and long-period seismic waves; development of high speed computational techniques.	365,000	
(10) Investigation of cross-correlation techniques for determining the direction of first motion "P" waves.	35,000	
b. Funds obligated through U. S. Geological Survey:		
(1) Investigation of crustal structure by refraction shooting.	310,000	

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
(Task 2 Cont'd)		
c. Augmentation suggested:		
(1) Increase effort in study of shear waves.		
(2) Increase number of study teams on aftershocks from one to three; includes study of area of high seismicity.		
(3) Model studies.		
(4) Equalization studies.		
(5) Improved seismographs and seismographic response including additional effort on strain gauge approach.		
d. Miscellaneous studies under consideration.	92,000	
Generation and Propagation of Seismic Waves - SUB-TOTAL	$2,350,000	$6,200,000
2. Detection Methods:		
These studies will be conducted by universities and commercial organizations. They will be aimed at the exploration of new methods and techniques for obtaining seismographic data.		
a. Funds obligated:		
(1) Study of characteristics of microseismic noise and signals recorded on the ocean bottom.	$ 121,000	
(2) Study of high frequency noise in wells at intermediate depth (1500 feet).	23,000	

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
(Task 2 Cont'd)		
b. Funds obligated:		
(1) Improved long-period detectors with greater discrimination against 4-10 second microseisms.	$ 50,000	
c. Funds to be obligated:		
(1) Direct digitizing seismograph, more stable long-period seismograph.	90,000	
(2) Deep well detection methods. To be obligated after report received in May 1960. Current AFTAC project.	450,000	
(3) Theoretical and experimental studies of large arrays.	400,000	
(4) Other improved seismographs.	100,000	
d. Augmentation suggested:		
(1) Large arrays - experimental studies including testing under adverse conditions and of noise characteristics. (See Task 2, c(3)).	*	

*Ten detector array being constructed as part of Geneva station.

(2) Deep hole detection, increase effort in deep hole instrumentation and add new deep hole site. (See Task 2, c(2))	*	

*Another program initiated in 1956 will be continued as part of VELA in FY 1961.

(3) Theoretical studies of explosions and their relation to detection.		

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
(Task 2 Cont'd)		
e. Miscellaneous studies under consideration.	16,000	
Detection Methods SUB-TOTAL	$1,250,000	$3,500,000
Generation & Propagation of Seismic Waves - SUB-TOTAL	$2,350,000	$6,200,000
Research Program TOTAL	$3,600,000	$9,700,000

C. Task 3 - Systems Development:

Includes construction of two different types of prototype detection stations for evaluation.

1. Geneva - type station: 225,000

   Construction of Geneva type seismic station with equipment specified by the Conference of Experts.

2. Geneva-type station with Berkner Improvements: 705,000

   a. Site selection and station design.

   b. Station construction.

   c. Selection, procurement and installation of equipment.

   d. Automatic processing of seismic data.

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
(Task 3 Cont'd)		
3. Augmentation suggested:		
a. Build and operate two unmanned stations.		
b. Construct and equip six operational type seismic stations for R&D and system evaluation purposes.		
(1) One simplified type.		
(2) Five of Geneva type (See Task 3, para. 2)		
c. Standardization of methods for reduction and analyses of seismic data. Requires international participation.		
d. Analyses center for data derived from systems tests.		
Systems Development TOTAL	$930,000*	$8,125,000

*$195,000 was diverted temporarily for studies under Task 6.

D. Task 4 - Program of Nuclear and Chemical Explosions

1. FY 1960 Program:		
Advanced preparations	$2,310,000	Not applicable
2. FY 1961 Program:		
Present estimates include 11-21 HE explosions and approximately 11 nuclear explosions to be fired in FY 61, 62. FY 61 costs include some expenses in preparing for FY 62 explosions. FY 60 money is to be expended in preparation for FY 61 explosions.	Not applicable	$42,990,000
Nuclear and Chemical Explosions TOTAL	$2,310,000	$42,990,000

PROGRAM	FUNDS	
UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
E. Task 5 - Direct Administrative Management and Operational Costs:		
Management Costs TOTAL	$ 50,000	$100,000
F. Task 6 - Short-term Studies*		
1. Feasibility of unmanned seismograph stations.	$ 100,000	
2. Feasibility of large (100 detector) arrays.	65,000	
3. Evaluation of identification criteria.	30,000	
4. Capability of net with additional manned stations.	In-house study	
5. Augmentation suggested:		
a. Evaluation of identification criteria and other short-term studies as required.		
Short-term Study TOTAL	$ 195,000	$ 300,000
G. R&D on On-Site Inspection TOTAL	110,000	$ 2,000,000

*Task 6 of Project VELA-UNIFORM covers the miscellaneous short-term studies which do not properly belong under any of the other tasks discussed so far. To date, the expenses incurred under this task include four short-term studies which are expected to be completed in the near future.

UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
H. Cost Summary:		
1. Standard seismological network: (Task 1)		
a. Equipment selection	$ 50,000	
b. Equipment procurement	700,000	
c. Technical installation		
d. Seismic data analysis center		
Sub-Total (Equipment being selected)	$750,000	$2,625,000
2. Research Program: (Task 2)		
a. Generation and propagation of seismic waves	$2,350,000	
b. Detection methods	1,250,000	
Sub-Total	$3,600,000	$9,700,000
3. System Development: (Task 3)		
a. Geneva type station	$ 225,000	
b. Geneva station with Berkner improvements, construction, selection, procurement, installation of equipment, automatic data processing	705,000	
Sub-Total	$ 930,000	$8,125,000
4. Program of Nuclear and Chemical Explosions: (Task 4)		
Sub-Total	$2,310,000	$42,990,000

UNDERGROUND DETECTION PROGRAM, Cont'd	FY 1960	FY 1961
(Cost Summary Cont'd)		
5. AFTAC Administration and Operational Costs: (Task 5)	$ 50,000	$ 100,000
6. Special Short-term Studies: (Task 6)	$ 195,000	$ 300,000
7. ARPA Reserve for Project VELA	$ 700,000	
8. R&D on On-Site Inspection	$ 110,000	$2,000,000
VELA-UNIFORM TOTAL	$8,645,000	$65,840,000

PART II

ADVANCED RESEARCH PROJECTS AGENCY
Washington 25, D. C.

5 May 1960

The Berkner Report items were taken from page 15 of, "Report of the Panel on Seismic Improvement; The Need for Fundamental Research in Seismology" dated July 1959. The VELA-UNIFORM cross reference listings were taken from the Project VELA Research and Development Status Summary of 25 April 1960.

Berkner Report Item	VELA UNIFORM Cross Reference
Individual Research Projects	
Equipping of selected stations with modern seismic equipment	A. Task 1; 1,2, 3 & 4
Study of shear waves	B. Task 2; 1b(2), 1 d (1)
Identification from aftershocks	B. Task 2; 1a(1), 1a(2) 1a(4), 1b(1), 1d(2)
Long-period surface and body waves	B. Task(2); 1b(4) first part
Geophysical investigation of crustal structure	B. Task 2; 1a(3), 1C(2)
Equalization experiments	B. Task 2; 1c(1), 1b(5), 1d(4)
Model studies	B. Task 2; 1b(3), 1d(3)
Noise studies and development of standards	* C. Task 3, 3c
Large arrays	B. Task 2, 2d (1), 2c(3)
Deep hole detection	B. Task 2; 2c(2), 2a(2), 2d(2)
Deep ocean seismograph	B. Task 2; 2c(4) in part. 2a(1)
Research computing facilities	B. Task 2; 1b(4) last pa
Library of digitized seismograms	C. Task 3; 3d in part
Theoretical studies of explosions	B. Task 2; 2d, (3)
Other	F. Task 6
System Development	C. Task 3
Unattended seismic stations	C. Task 3,a

Incl. B

Improved seismographs	B. Task 2; 1d(5), 2b(1) and 2c(4) in part
Portable or "throwaway" seismographs	Deferred on recommendation of Ad Hoc Group on Seismology
Automatic data processing	C. Task 3
System planning and field trials	C. Task 3
Implementation of detection system	C. Task 3; 1a; 2a, b, c; 3b(1), 3b(2)
Nuclear and HE detonations	D. Task 4; 1, 2
AFTAC administrative costs	E. Task 5

*R&D and evaluation only; operation system implementation to come later.

ATTACHMENT 7

PART I

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